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Hydrology and Earth System Sciences Discussions

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Interactive Comment

Interactive comment on "Agricultural groundwater management in the Upper Bhima Basin, India: current status and future scenarios" *by* L. Surinaidu et al.

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Referee 3 Response to specific comments

1. The most important problem is related to the simplicity of the numerical model. The authors describe the geological and hydrogeological contexts as highly heterogeneous, with variable layers, thicknesses, hydraulic conductivities and specific yields. They however use a very simple model with only one geological layer and uniform thickness, which does not really correspond to the weathered basaltic zone, as described in the paper. The specified stresses of the system (precipitation, recharge) are also described

as very variable according to the location in the modeled basin. The inputs of the model are however prescribed as uniform over the area. Moreover, the conceptual model is not explained in details.

Response: To the best of our knowledge, no previous study has modeled water budget responses at the basin–scale within a hardrock setting in India. This scope of work is important since watersheds and sub-basins do not exist in isolation and net transfers between them must be accounted for. The conceptual model is supported by the scale of the region modeled and assessed using the equivalent porous medium method. The Upper Bhima basin is an applicable candidate for this approach given the relative spatial uniformity of the properties over a large region. The decision to model the upper 50 m as weathered is described in detail in the response to reviewer 1 and further supported by satisfactory calibration using a range of hydraulic conductivity and Sy values and assessment using groundwater data from many wells spread across the region. As suggested, more detail of the model conceptualization and input parameters of the system are now included in the revised manuscript.

2. There is a constant boundary condition in the southeast edge of the model, but we do not know exactly where this condition has been imposed. This should be referred in a figure. Concerning the other boundaries of the model, no information is provided in the manuscript. Similarly, the calibration of the model is not described sufficiently. I would like to see the results presented in a graph or a map of the region to analyze spatial trends. Additionally, it is not clear if the simulations have been performed in steady-state conditions (p10666, line 15), or in transient conditions (11 years period?, p10666, line 22). In p10667, the model calibration is considered as sufficiently robust to be used for predictions purposes but the arguments of the authors are not presented. Therefore, an important work must be performed to improve the model and its description in the manuscript.

Response: These important points are now addressed. The constant head boundary condition is now shown in Figure 3 and more discussion about boundary conditions and calibration have been added in the revised manuscript. The calibration results of observed and modeled heads are now shown in figure 4. The model has been run under transient conditions and details have now been included in the manuscript to better explain and justify the modeling approach.

3. Recharge and pumping rates are calculated using simplistic empirical relationships. The equations are presented in the manuscript without explanations, what is not acceptable. We do not know how groundwater discharge is implemented in the model. Is groundwater discharge only possible along the constant boundary condition or also within the basin? Future climate change scenarios are also implemented with very simple relationships, without any change in the frequency of rainfall events. Are the temperature changes considered in the analysis ?

Response: These are an important set of points and the manuscript now includes a better explanation of the methods used. The derivation of annual groundwater recharge inputs was based on Groundwater Surveys and Development Agency (GSDA) recharge estimates for the watersheds within the region, weighted by basin-averaged annual rainfall. This approach was taken in order to correlate the recharge with rainfall. The derived a recharge coefficient of 11% concurs with other publications for this geological background. Groundwater extraction (based on a similar method of rainfall-weighted GSDA groundwater draft data) was modeled as negative recharge. The recharge and discharge are spatially uniformly applied at the appropriate times of the year as discussed in the manuscript. Whilst it is appropriate to question the validity of this method, the broad averaging for the scale of this model is supported by the degree of calibration, and water budget results of the modeling must be viewed from a full basin perspective. The constant boundary represents the lowest topographical part of the basin, where all groundwater is eventually expected to discharge. (The topography is mentioned in the following response.) Boundary conditions are now better explained in the manuscript.

Given the wide range of future climate predictions published and lack of detailed predictions at the model's scale, modeling specific future climate changes and their relationships to recharge etc must be dealt with in a general fashion and are inherently limited. We have not explicitly included variation to rainfall frequency or temperature since there is no appropriate way to predict how this will influence groundwater recharge. The forecast scenarios, although simplistic, include a matrix of different overall changes to rainfall inputs and extraction inputs that are not improbable. The forecasting covers a range of possible outcomes, including a possible worst-case outcome of reduced recharge and increased extraction. Whilst it is acknowledged that the forecasting rests on simple assumptions, this model has value for future water budget assessments to compare their predictions against.

4. The geographical context of the basin could also be described with more details. For example, I think that adding the topography to Figure 1 could be very useful for the reader to understand the system functioning. Similarly, describing rainfall using calendar month names could also facilitate the reading of the paper.

Response: More details have now been included. The topographic map which explains system functioning can be found in Immerzeel et al., 2006 and Pavelic et al.,2012 and rainfall referred according to calendar months in revised paper.

5. The authors use many local references in the manuscript. However, a lot of work has been performed in the topic of groundwater modeling and climate change scenarios. In a research paper, I think that a discussion should be provided about the work described in this paper in the context of past research.

Response: The local references have now been more appropriately included. A contextual discussion to groundwater modeling and climate change has now been included in the revised manuscript and related references have been included. However, almost all studies in the area focus on recharge estimation, watershed development, resource exploration and its availability. Few studies focus on security of supply associated with possible future changes in demand. None of the studies has focused on future avail-

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ability of water resources with increase in demand associated with climate change and watershed development. In the present study we addressed all unresolved issues concerning future sustainability of the groundwater resources in response to the effects of climate and demand-related pressures.

6. Referee has suggested to improve the format of the manuscript and to refer figures sufficiently in the text. It was also suggested to explain some sentences and acronyms in a better way.

Response: The revised manuscript has been carefully edited to improve the structure and grammar, and more references in the text are made to the appropriate figures.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 10657, 2012.

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