

Interactive comment on “A coupled modeling framework of the co-evolution of humans and water: case study of Tarim River Basin, western China” by D. Liu et al.

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This paper introduces a coupled modelling scheme for the description of hydrological, ecological, economical and social dynamics. Each of these disciplines have reached a good level in the interpretation of specific aspects, but there is a significant gap to fill in the combined use of specific sub-modules and in the understanding of their interactions. I appreciated the attempt proposed in this paper where the authors try to identify a scheme to tackle this problem. Nevertheless, I have a number of issues that require more attention.

1) My major concern is about the organization of the paper. The authors focus mainly on the model presentation, while the presentation of the results and the discussion are very limited. In the complex, the paper seems a brutal list of equations and functions without an adequate description of the motivation behind the model construction.

2) This one is a simple model, but it has more than 60 parameters and honestly I suspect that the calibration may be a real nightmare. I'm not surprised that model calibration over a run of about 50 year provide a good description of annual streamflow, but how can we be confident that you get the right result for the right reason?

3) Looking at the model validation, it seems that the data used contains very limited information. In fact, most of the variance is smoothed away by using annual averages. Under such condition, it is extremely simple to fit the model with the observed data, but errors are even more evident. In fact, the module describing the ratio of irrigated area was not able to describe the abrupt increase of irrigated areas in the last three years (see figure 9). This aspect is not considered in the paper, but errors may represent an inspiration for further improvements.

4) Model is applied at the annual time-scale. Are you sure that such scale is sufficient to represent correctly the processes of interest. In my personal experience natural processes may undergo abrupt changes that may be due to prolonged stress (see Manfreda and Caylor, Water 2013). This kind of processes cannot be modelled at the annual scale.

5) Looking at the results and more specifically comparing validation (e.g. Figure 9 or Figure 11) and projections (Figure 12.e or g), it seems that the data traces a trajectory that seems markedly different from projections in some cases. Which one is wrong? This aspect is particularly interesting and should not be neglected.

6) Finally, a discussion about limitations of the modelling approach and potential would be useful.

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Minor Aspects

Page 3914 – Line 15: Levins and Culver (1971) cannot be considered a paper in the field of ecohydrology, but Ecology.

Page 3920 – Line 16: I suggest substituting the term evaporation with evapotranspiration in this point and also in other places throughout the text.

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