

## ***Interactive comment on “A question driven socio-hydrological modeling process” by M. Garcia et al.***

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### **General**

This paper proposes a new approach for the development of socio-hydrological models: a question driven approach. In this process, the modeler is supposed to start by formulating a research question, which should be the basis for identifying key outcome metrics, formulating a dynamic hypothesis, and specifying model processes. The proposed approach is applied to the hypothetical case of Sunshine City, in which is dealt with the classic question of how to optimize the reservoir operation strategy for a large city. In this case study, the trade-off has to be made between two different reservoir operation strategies. A simple socio-hydrological model is developed- based on the

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proposed approach that couples the hydrological system to the human system and consists of a set of seven simple equations. Finally, the outcome of the socio-hydrological model is compared to the outcome of a conventional, non-coupled model.

In general I find the ideas of the authors interesting. I appreciate the effort to contribute to a generic framework for socio-hydrological modeling development, that might be widely applied to other basins and areas. By applying the proposed approach to a classical example, the authors demonstrate how one can use their approach. However, I do have some comments, questions and suggestions that might improve the quality of the paper and its message. Please find a list of comments below. If addressed and revised accordingly, this manuscript is suited for publication in HESS. If the authors have any questions, or like to discuss any of the comments, they are invited to contact me.

### Specific comments

1. I would like to know whether the authors believe that in the development of all socio-hydrological models in recent years, no one did this based on a research question. E.g., p. 8295, line 11: “much of the work to date. . . does not posit clear hypotheses or questions”. I think there might be a difference between work that didn’t use a research question of s/hypothes of is and work that didn’t present a research question/hypothesis. I think most model developers agree that a research question and hypothesis are essential in model design. Perhaps the authors can rephrase their statements, emphasizing on the difference between work that implicitly and explicitly use a research question and hypothesis.
2. The authors discuss some socio-hydrological models developed in recent years (e.g., Elshafei et al., 2014, Srinivasan et al., 2015, Di Baldassarre et al., 2013, Lui et al., 2014). First, I think some important models and approaches are missing (e.g., O’Connell and O’Donnell, 2014; Van Emmerik et al., 2014; Viglione et al., 2014). Second, if one is to propose a new approach, I think it’s important

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to demonstrate that the authors know, or define, what approaches were used in previous modeling exercises. Recent papers by Troy et al. (2015) and Blair Buytaert (2015) give an excellent overview of socio-hydrological modeling studies, their motivations and approaches. I suggest that the authors try to give a more complete overview of recent socio-hydrological modeling exercises, the approaches used, and the drawbacks or downsides of those approaches (according to the authors). Also, it would be interesting to take into account? some ideas, approaches, and perspectives from the recent special issue on socio-hydrology (Debates – Perspective on Socio-Hydrology) in WRR (see e.g., Montanari, 2015). This might create a clearer context of why the proposed approach is necessary to bring socio-hydrological model development forward.

3. The case study is nicely introduced, and I appreciate that (following the proposed approach) the authors pose a research question that is the basis of the modeling exercise. However, the dynamic hypothesis is posed very sudden. What makes this hypothesis dynamic? Why did the authors decide to apply a dynamic hypothesis? How do the authors expect the hypothesis to change over time, and what will trigger these changes?
4. I suggest to shorten the background information. I think it is more interesting to see how the proposed approach is applied to this case study, rather than read about every single detail of Sunshine City and reservoir management.
5. Page 8305, line 4: “Kandasamy et al., 2013” should be “Kandasamy et al., 2014”. Perhaps also refer to the modeling studies of Elshafei et al. (2014) and van Emmerik et al. (2014), which also revealed and discussed the system’s opposing economic and environmental forces.
6. The model equations can be described more clearly. Please introduce each symbol when describing the equations (incl. units). Although one can figure out what

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- all symbols mean using the table, it reads more conveniently if the symbols are mentioned in the text describing the equations.
7. The outcome of the coupled socio-hydrological model are compared with results from a non-coupled (simple) model. However, there is no description of this model. I think this is essential. This should also include some justification for the use of this model. I assume that this classical problem has been solved in many different ways, so a more elaborate discussion would be interesting. Especially to compare this with the outcome from the presented approach.
  8. I'm wondering whether it is necessary to present the outcomes of three trials. In my opinion, the paper is not about the developed model, but about how this model is designed and evaluated. To do this one wants to run the model with different settings for  $K_p$  to see how the balance of the system might shift. This can then be compared with the model outcome from a 'conventional' approach. Eliminating two trials might perhaps make the paper a bit more structured. I leave it up to the authors to decide whether presenting the three trials are necessary.
  9. I would expect that the discussion would mainly be about comparing the results gained from using the proposed question driven approach and a conventional approach. The whole discussion about the behaviour of reservoirs and the differences between SOP and HP is not new or relevant, within the perspective of this paper. I would suggest to explicitly start with "The model outcome of the question driven socio-hydrological model suggest. . .", continue with "The model outcome of the conventional model approach suggest. . .", followed by a clear and concise comparison of the two. Now, it's a bit unclear to me what the discussion aims to address.
  10. At the end of the discussion, the authors discuss the use of a socio-hydrological model versus the use of a simple non-coupled model. I think the authors make

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- a strong case here, and I would suggest to emphasize on this finding in the abstract, introduction, and methods too. Examples of how socio-hydrology advance our understanding of systems are valuable for the whole community.
11. I appreciate the authors' critical evaluation of the used case study. When reading the paper I found myself thinking that this case study is "simplified and simplistic", which the authors later on acknowledge. Perhaps it might be nice to already mention this at the beginning of section 3, at the description of the case study.
  12. Why did the authors choose to apply the proposed approach only to a hypothetical case? Of course the analysis of toy models might lead to significant and important insights (e.g. Di Baldassarre et al., 2013), I would think that this new approach is especially of value for real life situations. I leave it to the authors to decide whether to (1) include an application to a real case study, or (2) discuss the choice to only apply to a hypothetical case.
  13. Page 8316, lines 12-21: a bit repetitive. As part of the conclusions, I would emphasize on what this paper adds to the current spectrum, instead of focusing on what is lacking in previous work.
  14. I don't see a final conclusion that shows that your question driven approach is superior to a conventional approach. I suggest to include at least one crucial finding that makes the case for your presented approach (rather than only concluding that socio-hydrological modeling leads to new insights).

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