

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

Anonymous Referee #2

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GENERAL COMMENTS:

This manuscript outlines a question-driven framework for the formulation of socio-hydrology models. The authors draw on key ideas in the social-ecological systems literature to illustrate how a backward reasoning approach may be applied to developing a coupled human-hydrology model capable of examining the question of optimal reservoir operation policy. The approach commences with the definition of a key research question, which then forms the foundation from which outcome metrics and a dynamic hypothesis are identified, and subsequent fundamental processes and variables defined. The authors focus their application on a hypothetical case study. They devise a coupled socio-hydrology model consisting of a set of seven equations designed to explore the relative merits of two competing reservoir management strategies

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over a 50 year period. The insights gained from the coupled model are then compared against those provided by a traditional model, which illustrates the improved utility of a coupled model in this regard.

Overall, I found the manuscript to be well written and interesting with well-constructed arguments. It offers an alternate framing approach and a novel application of a coupled socio-hydrology model to the pertinent issue of reservoir management policy. I do however find there are a few key areas where the manuscript may be improved. I believe that with certain revisions this manuscript is of interest to HESS readers and an important contribution to the growing body of socio-hydrology literature.

SPECIFIC COMMENTS:

1. I feel that the manuscript presents interesting ideas in terms of applying a combination of two SES frameworks to a socio-hydrology question and I would agree that the application of a backward reasoning approach is indeed novel in this space. However a shortcoming of the paper is the seeming omission of the now growing body of socio-hydrology (SH) literature. Section 2 reviews the relevant hydrology and SES literature, however makes scarce mention of previous work in the SH space, other than to say that most of the work does not posit clear hypotheses or justification as to model structure, scope and scale. I am not convinced this is the case as arguably all conceptual and deterministic models developed to date for human-water systems are necessarily formulated on the basis of dynamic hypotheses, with some more grounded in theoretical hypotheses generated by the literature, while others are more guided by observations (e.g. Carey et al (2014); Di Baldassarre et al. (2013a, 2013b, 2015); Elshafei et al. (2014, 2015); Hale et al. (2015); Liu et al. (2014, 2015); Srinivasan (2015); Troy et al. (2015); van Emmerik et al. (2014)). I would urge the authors to acknowledge/ review previous work more fully and clearly distinguish how this approach is different, otherwise it reads as though the authors believe the framework presented here appears in a vacuum. I would also suggest the authors have regard to the recent WRR Debates series and perhaps illustrate how this approach addresses some of the current

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challenges being discussed (Montanari, 2015).

2. It is not immediately apparent why a hypothetical case has been chosen when the approach seems intuitively geared to a real world case. I would suggest providing a clear justification for this decision in Section 3.

3. Section 3.1 is very well written and provides a strong justification for the dynamic hypothesis in the literature. Please add a sentence in the opening paragraph (p. 8299 line 12) to explain why the third characteristic of water shortages (i.e. length of the shortage) is not relevant in this examination. Given the statement on p.8303 lines 4-6 regarding the importance of the duration of the shortage in terms of galvanizing conservation behavior, it is worth noting why consideration of this component is not of interest here.

4. In the first and second paragraphs of section 3.2 the authors explain how the research question is used to derive the key outcome metric and processes. However, although the derivation of the outcome indicator is clear, the transition to the definition of the 3 key processes is quite abrupt. Passing reference is made to the SES framework, without any clear explanation on how this has been used in this instance. Given this is arguably the key premise of the paper, i.e. the use of a question-driven modeling approach, I would urge the authors to augment this leap with a few sentences offering a brief explanation of the framework and tying in specific examples as to how it has been employed to arrive at the 3 processes. Why is population growth included and not economic growth for example? In addition, the authors state on a number of occasions (as early as the abstract) that the merit of this approach is that it provides clear guidance on model scope and detail, however this does not come across in the description of how the processes/ variables are ultimately defined.

5. It is interesting to note the broad similarities of Fig. 5 (and the accompanying narrative) and previous feedback loops used in recent SH literature - i.e. Elshafei et al. (2014, Fig 1) and Sivapalan (2015, Fig. 2), defined as positive (Economic-Population)

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and negative (Community Sensitivity) feedback loops. These are also referred to in terms of destructive and restorative forces in Liu et al. (2015). In the case of this model, population is effectively driving the positive feedback loop/ destructive forces, while shortage awareness is driving the negative feedback/ restorative forces. I believe the authors could enrich their discussion with a more objective comparison of the work presented here with previous work, i.e. acknowledge the similarities and draw parallels, and note the points of difference.

6. p. 8305 lines 1-5: This is an unsubstantiated assumption and in my view not strictly correct. The Murrumbidgee basin is an example of a situation where the cumulative negative consequences of development stimulated water conservation behavior, rather than being an example of a weakening link between economic growth and water demand per se. In this case water demand was overshadowed by other environmental considerations, in much the same way as your approach posits in Fig. 5. I would suggest the authors find a more compelling example of the relationship they are suggesting, or perhaps adopt a different way of justifying the exclusion of the economic process.

7. p. 8305 lines 9-11: This is a fair point in order to reduce unnecessary complexity. However, it may be worth noting it in the discussion as part of the model's limitations given recent studies are finding that agent-based models are important in the examination of human behavior in coupled human-nature models (Kinzig et al. (2013); Tavoni et al. (2012)).

8. p. 8305 lines 23-26: Does this mean that land use changes are also ignored?

9. In equation 3 it appears as though population dynamics respond to any shortage awareness, rather than being limited to extreme cases as suggested earlier in the manuscript (see p.8304 L22, p.8306 L19). Is this correct? And if so, why is this approach taken given earlier discussion regarding "extreme" events?

10. Given that the manuscript's focus is on a novel approach to the development of a

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SH model, and its subsequent application to a classic water management question, I think the discussion should really begin with a primary focus on the contribution of a question-driven modeling approach, as opposed to the merits of competing operating policy strategies. I would suggest revising the discussion to emphasize the superiority of employing a coupled SH model versus a traditional model in this application, and the novelty and efficacy of the model formulation approach.

TECHNICAL CORRECTIONS:

1. References to "the system" would be better phrased as "the coupled system" to make clearer the distinction between traditional and socio-hydrological modeling approaches - e.g. p.8292 lines 3, 9, 10; p.8312 L13; p.8316 L11.
2. p. 8294 line 9: I'm not sure that the Wheeler et al., 1993 reference needs to be repeated on lines 14 and 16 given it is referenced at the outset with regard to all modeling approaches discussed in the paragraph.
3. p.8296, L10: Arocha et al. 1993
4. p.8296, L24-25: This sentence does not make sense - is there an extra "on"?
5. p.8301, L2: "consumers' attitudes"
6. p.8305, L18: "clearly change over the course..."
7. p.8305, L26: delete "of" in "of the selected of operating policy"
8. p.8306, L22: delete "a" in "while the logistic function is a commonly used to model resource constrained"
9. p.8313, L22: perhaps substitute "with a" with "using" i.e. when we compare SOP and HP using a SH model...
10. p.8315, L14: "innovatively"
11. Fig. 4: please define "gpcpd" for ease of readability.

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12. Check date inconsistencies of references: Gal (1972 vs 1979), Kanta and Zechman (2013 vs 2014), Sivapalan (2011 vs 2012)

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