Dear Reviewers,

Thank you for your helpful comments. Our response is broken out such that the reviewer's comments are in normal font and our response in italics.

All the best
Tara Troy, Megan Konar, Veena Srinivasan, and Sally Thompson

Reviewer 1:

Summary: This paper synthesizes a collection of studies, mostly from the special issue in HESS/ESD, "Predictions under change: water, earth, and biota in the Anthropocene," and brings out certain key elements that in the opinion of the authors dominate these studies, such as one vs two directional coupling, type of socio-hydrological data used, norms and ethics as feedbacks, value laden nature of socio-hydrological research, etc.

Comments: This paper is an interesting contribution to the special issue. The discussion of one-way vs two way feedbacks and dynamic connectivity is quite interesting. I have only few concerns, which I hope would help the authors to provide a more balanced synthesis.

We thank Reviewer 1 for the positive summary. Throughout the Review, it appears that our interpretation of the term "synthesis" differs from the reviewers. We wish to address this upfront.

Reviewer 1 describes the goal of this paper as being to: "bring out certain key elements that in the opinion of the authors dominate these studies." That is, Reviewer 1 interprets our use of the word "synthesis" as primarily meaning a "summary" of the papers in the Special Issue. In several additional comments, Reviewer 1 raises the concern that we incorporate issues that were not clearly articulated by papers within the Special Issue, and asks whether this is appropriate in a synthesis paper.

Our motivation in writing this paper is to provide a critical evaluation of the papers in the Special Issue dealing with sociohydrology, including addressing the conceptual and methodological gaps and opportunities that resulted from this evaluation. Thus, our goal was to go beyond a simple summary of the papers within the Special Issue. We agree that this is necessarily a subjective exercise that led us to prioritize issues that we consider important. However, we believe this "critical synthesis" leads to a more useful and forward-looking paper than one which was constrained to simply summarize existing works. For this reason (and others, see below), we did not prioritize an exhaustive summary of the papers in the Special Issue, and we have included topics of discussion that we see emerging from gaps in the existing literature, rather than simply reflecting the points of view made already by other authors. In addition, some of the papers in the

Special Issue were more focused on traditional hydrological prediction rather than the feedbacks between human and natural systems, and these were not included in the synthesis.

We will clarify the paper's intent and our interpretation of what a "synthesis" entails in the revised manuscript.

1) The statement in their abstract that socio-hydrology can be embedded in socio-ecological studies has nowhere been substantiated by the synthesis.

Thank you for raising this important point. We agree that the relationship between a socio-ecological system and a socio-hydrologic system has not yet been demonstrated in the literature, and it will be removed from the abstract.

We raise the issue in the main part of the paper not because it has been addressed, but because its omission is problematic – ignoring the socio-ecological context of any socio-hydrological problem risks over-simplifying the web of inter-relations between water and society, and ignoring established theory and methods. Going forward, we think this is an important avenue for socio-hydrology researchers to consider.

Rather than looking within the Special Issue to illustrate this point, we can take many examples from the socio-ecological systems literature. Picked here, more or less at random, is a causal loop diagram intended to diagnose factors leading to sustainability in the management of the Cat Ba Island Reserve in Halong Bay, Vietnam (Nguyen, N. C. and Bosch, O. J. H. (2013), A Systems Thinking Approach to Identify Leverage Points for Sustainability: A Case Study in the Cat Ba Biosphere Reserve, Vietnam. Syst. Res., 30: 104–115. doi: 10.1002/sres.2145).

Water factors into this question – the utilization and availability of groundwater influences the biodiversity and attraction of the island to tourists (Figure below). However this causal relationship is embedded within many additional and complex feedback loops, arising from the intersection of tourism infrastructure and revenue with agricultural practices and investment, population dynamics and conservation practices. Isolating the effects of water on the sustainability of this socio-ecological system is a non-trivial challenge, and an attempt to focus purely on water without somehow controlling or accounting for the other influences on revenue, etc. at Cat Ba Island would be fallacious.

Clearly situations can be imagined in which the connections between social dynamics and water dynamics are simple. In today's complex society, however, it is hard to conceive that these situations are the norm rather than the exception, and bringing awareness of socio-ecological systems theory to the table when considering socio-hydrology is likely to be important for the majority of case studies.

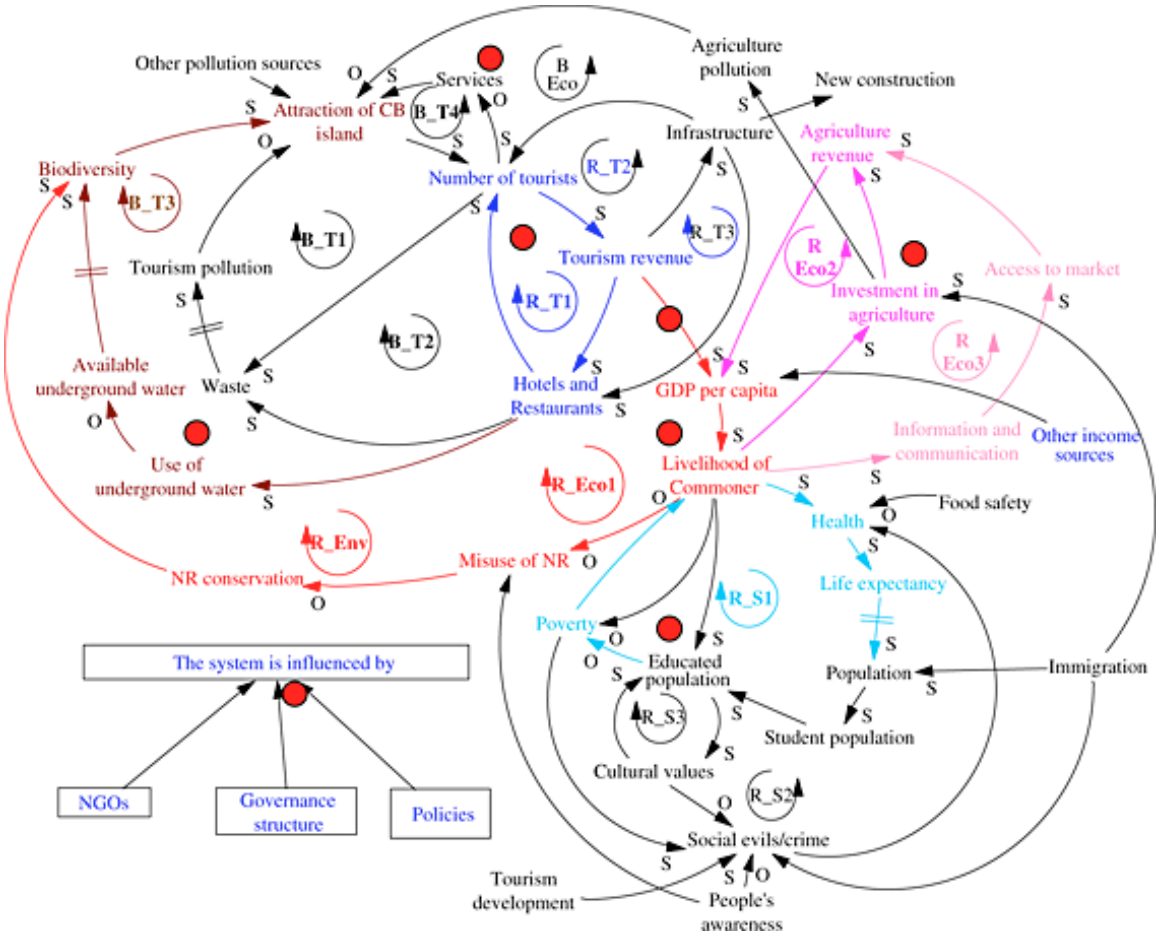

Figure 3 from Nguyen and Bosch (2013) demonstrating how water/hydrology can be just one component of a larger SES framework.

2) The paper perhaps may want to provide an exhaustive review of the special issue first before embarking on the synthesis. This will allow potential readers to put the synthesis in context of cited literature. At present it appears that the synthesis is selective and often the paper gives an impression of being an opinion piece rather than an unbiased synthesis of the special issue. References to the articles from the special issue appear to be selective.

As explained above, the goal of this paper was not to provide an unbiased summary of the papers in the Special Issue, but instead to undertake a "critical synthesis" of conceptual and methodological issues that emerge from a review of the papers, including highlighting gaps that we think are important. We will clarify our motivation in the revised manuscript.

Further, many papers in the Special Issue are not deeply socio-hydrological in nature, but instead address questions such as anthropogenic climate change. These papers maintain the traditional paradigm of treating humans as exogenous agents influencing hydrological response, rather than as a component of an integrated system. These papers – while undoubtedly valuable scientific contributions – are not clear examples of socio-

hydrology research, so we do not focus on them in this paper. In the final version of the paper, we will ensure all those papers that are socio-hydrological in nature are included.

3) Do we need a synthesis of the special issue to discuss the challenges faced by socio-hydrological research methodologies? Models will always be value laden, or that finding appropriate data will always be a challenge irrespective of the field of research.

Thank you. Methodological reviews that are specific to individual fields are valuable for practitioners in those fields. It is universally true that common methodological strategies across fields will have common limitations and challenges: this does not mean that methodology should only be assessed in abstract terms and isolated from the proposed applications.

4) Section 3.1 and 3.2: if socio-hydrology is limited by data, to what extend can we then use techniques from nonlinear dynamics theory (including identification of dynamic connectivity, threshold behavior, and multiple stable states) or from econometric literature on causal inference? These techniques do not work well when data is scarce. Further, do the suggestions of the authors that we should use complex system science and econometric techniques in socio-hydrology emerge from the synthesis of the special issue?

We agree that nonlinear dynamics and econometric techniques are data hungry, and as such might be better suited to some socio-hydrologic problems (e.g. those that emerge in a contemporary setting, when remote sensing, "big data", and distributed observation platforms offer the capacity to obtain large datasets) than to others (e.g. historical reconstruction). For this reason, we include the first sentence of Section 3.2 ("If the data availability and reliability challenges can be overcome…").

Our point in highlighting these methods is not to insist that they are the only approaches that can be applied in socio-hydrology. Instead, it is to note that to date there do not appear to have been any serious attempts to use these methods to address this problem (at least in a study that identifies itself as being socio-hydrological), despite their potential promise.

Again our identification of these methods as needing discussion emerges from the observation of a "gap" in the methodologies in the Special Issue, not from a "summary" of techniques used. This will be clarified in the revised manuscript.

5) Use of econometric methods in reducing bias in estimation of sociohydrological model parameters is an opinion that I share with the authors. But the assumptions behind existence of estimation bias in econometric models are based in microeconomic concepts such as utility maximization. Techniques such as instrument variable regression have been proposed to remove such biases, assuming that agents, for e.g., maximize their utilities. Yet the authors suggest the use of econometric methods for causal inference alongside the use of nonlinear system dynamics theory that does not have any microeconomic underpinning. The synthesis of the authors suggests that system

dynamics based socio-hydrological models are the only types of models in the special issue. I wonder if one can then use econometric methods for causal inference using such models.

To elaborate further, consider the flood-human model of Di Baldessarre et al. (2013). The movement of population center to or away from a river corridor and human actions of raising a levee are given by apriori specified functions. Corresponding parameters of the functions are accordingly defined. While such specifications provide powerful insights, the nature of bias in estimating its parameters is not clear unless there are certain underlying models that specify how the choices of population movement and raising of levees are made. Without clarity on underlying choice hypotheses, it is difficult to apply instrument variable techniques such as 2-stage regression to remove parameter estimation bias. Efforts are currently underway to explain coupled human flood systems using growth theory, expanding the possibility to understand and remedy biases in inferences of causal relationships. The authors may therefore want to further clarify when to use econometric methods for parameter estimation of socio-hydrological models.

Thank you for allowing us to clarify this important point. We highlight the potential of applying methods of econometric causal inference to socio-hydrology settings in Section 3.2. These tools are used to understand if a causal relationship exists between variables in complex systems, and, if so, to quantify its magnitude. So, we suggest that tools of econometric causal inference have a large potential application to socio-hydrology systems. It is true that the estimate obtained from a causal inference estimate could be used to parameterize a socio-hydrology model, but we do not suggest this is in Section 3.2. We instead highlight the opportunity of applying these state of the art statistical techniques to empirical data to gain causal understanding in socio-hydrology.

Tools of causal inference in econometrics do not make assumptions about underlying system microeconomics. Thus, these tools can be readily combined with other tools of analyses – such as complex systems theory – without worry about incompatability in the underpinning assumptions. The assumption underpinning each specific technique in causal inference is distinct, but typically the tools assume that "pseudorandomization" has been achieved and a causal interpretation is warranted. For example, in the case of instrumental variables -- one type of causal inference tool in econometrics -- it is assumed that the instrument is NOT correlated with the error term in the explanatory equation, thereby correctly identifying the causal impact of treatment. No assumptions are made about underlying system mechanisms, such as maximization of utility amongst agents.

We think part of the confusion stems from our use of the term "econometrics". We will instead use the term "causal inference" in the revised manuscript.

Reviewer 2, Maurits Ertsen:

We would like to thank the Dr. Ertsen for the helpful comments. Our response is broken out such that the reviewer's comments are in normal font and our response in italics.

The paper presents a nice overview of the different contributions on sociohydrology (SH) in the special issue of HESS/ESD. As such, I only have a few remarks on elements in the text which might ask the authors to push their argumentation just a little further.

Thank you – we have found these prompts to clarify our thinking, expression and logic very helpful.

First on the issue of complexity. I am not too sure the general definition of complexity is very helpful. The idea of multiple interactions is not so clear anyway, it is how these are defined and conceptualized.

The complex systems idea will be expanded in the text. Based on the handwritten comment on the PDF, it appears that the authors and the referee think about the meaning of "complexity" differently and this will be clarified. We were referring to complex systems as one would in complex systems science, not the typical vernacular meaning.

On page 3322, two interesting remarks are made that I would see as examples of phrasing (and framing) that would require some more thought. How can there be scale mismatches between systems (line 15)? Either systems are related or they are not, and I am certain not all the processes are to the liking of all, but mismatches suggests there would a good match possible. For whom is that to judge?

An example of this would be a time scale mismatch, and by this term, what we mean is that hydrologic processes may operate at one time scale while societal dynamics on another. The simplest example might be flooding and flood response. The flood can occur on the order of hours to days, whereas the response to flooding can occur on the order of weeks to months or years, as has been seen with Hurricane Katrina in New Orleans. This point will be expanded/clarified in the revised text.

In addition, why is two-way coupling necessarily a slow process (line 25). That seems to presuppose certain types of changes.

We thank you for this comment, as we agree that this was not expanded enough. In several of the studies that demonstrated two-way coupling, the work required decades of observations. For example, in some cases, we saw degradation of the water system occur over decades until the human system responded to change their behavior (the Murrumbidgee is an example of this).

These opening remarks from my side link to the issue of one-way or two-way

couplings. I would argue that all our SH systems are two-way coupled, or, as the authors correctly state, there are always multiple couplings. I would like to draw the attention to the concept of Human Niche Construction, which builds on the concept of Niche Construction in stressing that in changing their selective environment organisms change themselves too. Human NC simply argues that humans do so as well. In stressing the importance of human agency, HNC comes close to the concept of Evolutionary History (EH). Evolution is everywhere, happening all the time and humans have played an enormous role – conscious or not – in shaping evolutionary processes. HNC and EH relate changes across four dimensions: 1. Material environment – modified by human agency; 2. social arrangements – when modifying the environment and responding to the changes; 3. genetic structure of the human group – as a result of modifications; and 4. genetic structure of other groups than humans. Now, whether these changes are short term or not, and how extensive they are, is not easily to defined before any research. The issue which level of coupling to go for would but only be a matter of methodological possibilities or limitations to engage fully with the fully coupled system. There is only one way to go in theory, but practical limitations might require distinguishing between more or less integral coupling. This would mean we need to think about two-way coupling, water-human one-way coupling, and water-human one-way coupling as methodological issues, not as conceptual issues.

This is an interesting theory of which the authors are unfamiliar, but it does seem like there are parallels with sociohydrology. It would be an open question about genetic changes and studies about this would certainly be interesting although beyond the disciplinary boundaries of those currently conducting sociohydrologic research. We agree that coupling needs to be considered as both two-way and one-way, but this needs to be done both conceptually and methodologically, not one or the other.

The paper suggests a few times that whether areas are wet or dry matters (page 3324 for wet, 3325 for dry). I have no problem with bringing material conditions in the analysis of SH, not at all I should say, but the whole concept of SH forces us to rethink what dry and wet mean. The material conditions are no longer external anymore. Quite often, the issue is not wet or dry, but linking different rhythms and the manipulations to realize certain rhythms. The paper also suggests a few times that wetness or dryness shapes behavior or preference (page 3339 for example); a similar relation between rich and poor people's preference is suggested on page 3331. The observation that certain sites have something does not make it a preference. Collective outcomes are not to be confused by people's outcomes. Societies do not make any change, people do. We should not confuse outcomes with actions. Obviously, I am flattered that the authors use in their conclusion the choice between two approaches as discussed in one of my own contributions to the field , but I would like to suggest that several approaches in the special issue are either not making that choice or do things that go against what I wanted to suggest when making the statement in the first place.

Thank you for raising this important point. We agree that a simple description of the

state of water resources ("material conditions", as you call it) is often inadequate, as it is with people's behavior. Indeed, it is often a description of the state variable of interest relative to other spatial or temporal locations that is the driver of system dynamics. These spatial and temporal heterogeneities are critical to both hydrology and social systems, providing a key basis for socio-hydrologic coupling.

We will incorporate this important critique into the revised version of the paper.

A main concern I have about the field of SH (and social complexity in general) is how human agency is taken on board. This issue is discussed in the review paper, but I would suggest some more on it along the lines below – either in agreement or disagreement.

Many times, predefined responses are used, or known responses are copied by an algorithm. This includes quite a few studies on the Murrumbidgee system, but also the work on flooding. The unraveling of feedback seems to rather difficult in such work, as the feedback mechanisms have been predefined. What is there to unravel when the outcome is already known? Basically, the approach that shows that it can mimic behavior that was expected (which is pretty good perhaps) does not provide a way to be surprised. The problem in complexity sciences to me is the two-way issue of assumptions and pattern-repetition. What we think will happen is modeled to happen. This is a huge problem and we should strive for avoiding doing exactly that in SH. Whether economic sciences (or sociology for that matter) will help much is not clear to me. Several fields of economy have been pretty successful in using predefined behavior as input – claiming the predefined agency to be the desired one was well.

The econometric methods for causal inference suggested in this paper should help with this. These methods are used to determine causality in complex systems. Although they emerge from the economic sciences, they can be applied to a range of scientific studies. We suggest that this methodology be employed more in future work for exactly the point you raised – it is currently difficult to know if the models work because the feedbacks are pre-determined and prescribed.

If there are fields of scholarship the SH community could engage with it is (environmental) history and archaeology. In those disciplines, ideas about the value of data from the past, from human-environmental interactions, about proxy's and analysis of change, are much better developed than in the hydrological community – and if you ask me also in much of the complex sciences field, which is heavily driven by economy and psychology.

This suggestion will be incorporated in the Research Methodologies section as both of those fields have a lot that could contribute to sociohydrology.

In addition to the written review, a PDF was included with hand-written

remarks. Not all of these remarks require addressing as some were included in the formal review and others are in agreement with what was written, but those that do are included below.

Page 3323 what does the "dynamic" offer extra…

This point will be clarified. The dynamic refers to coupling that only arises under certain conditions. For example, humans may be unaffected by their region's hydrology until a catastrophic event. This was seen in the northeastern US, which was largely oblivious to flooding until Hurricane Irene and Superstorm Sandy.

Page 3324, water rich: *this point will be clarified.*

Page 3325, statistical vs. mechanistic: *We do not see this as something that is either/or, rather they are complementary. Statistical relationships need a mechanistic explanation, and a mechanistic theory requires evidence, which could be a statistical relationship.*

Page 3326: *the sociohydrologic system transitions will be described.*

Page 3328: *It will be made clear that these points are only based on the special issue, and as such constitute a hypothesis, not a conclusion, that requires further study. The phrases that the referee noted with a "Why?" will be expanded. This is true for all the "Why" questions throughout the document.*

Page 3331: *The language in the second noted will be corrected.*

Page 3332: *We disagree with the referee's position that complex system science is solely based on assumptions and pattern repetition. There are mathematical techniques available that can establish causality and do not require a priori assumptions of relationships, such as those introduced in Sugihara et al (2012), based on non-linear state space reconstruction.*

Reference: Sugihara et al (2012) Detecting causality in complex ecosystems, Science, 338:6106, pp. 496-500.

Page 3338: *We fully agree with this point and are aware of a collaboration of researchers currently studying and exploring the community sensitive state variable.*

Reviewer 3, Giuliano Di Baldassarre

We would like to thank Dr. Di Baldassarre for his insightful review. Rather than address his review by point by point, as was done with the other referees, we will instead address the broad issues he brings up as this is more applicable.

Basic vs. applied science

This is an excellent point that was glossed over in the submitted manuscript.. We agree that sociohydrology is still in the basic science development stage with a significant need to pull together case studies with comparative analysis and the development of theory and stylized models (steps 1, 2, and 3 as outlined in the review). In Sivapalan et al. (2012), they called it a use-inspired science. For sociohydrology to reach that goal, it requires both basic and applied science, as the theory cannot and should not exist in a vacuum, apart from water science practitioners. However, before it can be out to use it practice, it does need to develop the basic science component of knowledge building, as Dr. Di Baldassarre notes.

Stylized/toy models

The sentence that the referee objects too will be softened, as he makes a good case that for some models typical validation is not possible nor applicable. We agree that stylized models present an opportunity to advance our understanding and as such are potentially powerful tools for understanding the system dynamics. We believe we might be disagreeing on the meaning of the word "validation" in this context. We agree with the referee's point that these models can not be validated in the same paradigm as traditional hydrologic models; rather the model validation should involve ensuring it captures the system dynamics. This will be made clearer in the revised manuscript.

However, we do not currently have a community consensus on what stylized model validation (or diagnosis, using the referee's language) should look like. One reason for this might be the lack of studies thus far that do exactly what Dr. Di Baldassarre suggested: iterating on case studies, comparative analysis, and identifying salient facts or feedbacks. Until this work has been done, these stylized models rest, in some cases, on hypotheses of coupled human-natural system dynamics that are not yet theory. If we can build to theory as a community, then we will be able to have more faith in these models, knowing that the results are not simply an artifact of imposed model structure.

Additional references

We will include the points made by Ahlers et al. (2014) and McDonald (1989) in the revised paper. In addition, we will incorporate the WRR Debates papers. Thank you for pointing these out.