Authors' Resubmission and Response to the Editor: Hydrol. Earth Syst. Sci. Discuss., 11, C161–C163, 2014.

The authors are grateful for the opportunity to resubmit our revised manuscript: "A prototype framework for models of socio-hydrology: identification of key feedback loops with application to two Australian case-studies" by Y. Elshafei et al. We would like to sincerely thank both reviewers as we feel the manuscript has significantly benefited from the suggestions made.

The main revisions we would like to highlight are:

- The abstract has been substantially revised, in line with the request to include greater detail as to the specifics of the conceptual framework and case study results.
- Sections 1-2 have been substantially revised, in line with requests from both reviewers to make certain discussion points more concise, elaborate on the systemic approach adopted in our framework, and reshuffle subsections such that key messages are brought to the forefront and reinforced throughout.
- The results of a stylised model are reported for each case study, in line with the request to numerically demonstrate how the framework could be parameterised such that it may be more effectively evaluated. Please refer to the new section 4.3 (Discussion of Preliminary Results), Fig. 6 and 7 (which show model input and output graphs for each case), and the substantially revised Table 1 (which details the model assumptions, parameterisations, closure relationships and data sources used).

Pursuant to our detailed responses to each of the Reviewer comments made in the public discussion, detailed information as to where and how each comment or suggestion has been addressed in the manuscript revision is provided below.

1. Review comments by Christopher Scott:

General Comments:

Comment 1a: This manuscript addresses a key concern of socio-hydrology: better understanding and a refined conceptual approach to the two-way coupling of human-water systems. It proposes a three-parameter conceptual model to offer improved explanatory insights on the dynamics of agricultural catchments. The three parameters are a) climate/ aridity, b) socioeconomic development, and c) political dynamics. The authors postulate co-evolutionary human-water dynamics in two catchments in Australia, and seek to draw out more generic implications of their approach for other contexts globally.

Response: By way of clarification, the authors would like to note that the conceptual framework is made up of *six* key catchment components: namely, catchment hydrology, population, economics, environment, sociology (i.e. sensitivity variable, V), and response (i.e. feedback from the human system by way of management or community action, related to χ). Among these the model includes numerous parameters, and one of the novel aspects of the framework is the inclusion of the three "macro-scale" parameters, as highlighted in the comment, as part of the sensitivity variable (V). These three macro-scale parameters were included to enable the framework to be applied across climate, socioeconomic and political gradients, thereby making it more generically applicable. This functionality is achieved by normalising for differences in each of these parameters based on a catchment's location.

In sum, the authors believe that the conceptual framework makes three principal novel contributions:

- The formulation of the Community Sensitivity variable, V, which takes into account economic and environmental circumstances within a catchment.
- The inclusion of the Behavioural Response variable, χ, as the feedback mechanism linking the human and hydrology systems.
- The introduction of the macro-scale parameters that enable comparative studies to be undertaken across geographical gradients and between contextually different problems. Other locally relevant parameters must also be assigned to fully implement the model framework as are discussed in the case studies.

We have highlighted these points in the revised abstract.

Comment 1b: Based on extensive review of concept and theory in the literature, including references to coupled human-natural systems and social-ecological systems, this manuscript contains significant conceptual grounding. However, I find it to be vague, at times confusing, for its lack of empirical specificity. Further specific comments are offered below and in the annotated manuscript I am returning with this review.

In sum, I consider that after major revisions this could be an important contribution to the field of socio-hydrology.

Response: We have substantially revised sections 1-2 and made the review of concepts and theories in the literature more specifically tied to the arguments constructed. We have also added numerical results of a stylised ("toy") model for both case studies to provide preliminary empirical support for the theories outlined (please refer to section 4.3, Table 1, Fig. 6 and 7). Please see our responses regarding specific revisions below.

SPECIFIC COMMENTS:

Comment 2a: The abstract only loosely presents the actual content of the paper. To be more useful to the reader, this needs major revision, including reference to the two case studies.

Response: We acknowledge that the abstract was previously too general. We have therefore expanded it to include greater detail regarding the six key components of the conceptual framework, the three novel contributions it makes, and included brief descriptions of the Sensitivity variable and the response component. We have also made the distinction between catchment components and macro-scale parameters clearer by specifying each of the six catchment components and introducing the macro-scale parameters as an additional functionality that enables the framework to normalise across study sites. Finally, we have included a brief statement regarding the preliminary findings of the stylised model as applied to the two case studies.

Comment 2b: The sections all the way through and including 2.2 (totalling 14 pages) read more as a term-paper literature review (A said this, B said that) than they do a focused enquiry on the specific question(s) at hand, i.e., rationales ("drivers") for human responses to catchment hydrology dynamics. There is much of use in these pages; I am not suggesting they be omitted (with the possible exception of IWRM, unless this is much more clearly targeted to the discussion points raised by the authors). Instead, I am suggesting a substantive overhaul of sections 1 – 2.2 to address: a) processes of 'reverse' feedback of human responses and decision-making/ policy that stem from biophysical and agricultural processes in a catchment context, and b) identification and prioritization, among many parameters, of the three selected for further scrutiny (aridity, socioeconomic, and political gradients).

Response: The authors would like to thank the Reviewer for this suggestion and we believe the manuscript has benefited from the restructuring of arguments presented. We have substantially revised sections 1 and 2 as stated above, incorporating the suggestions of both reviewers. In an effort to increase conciseness, we have deleted material which was not specifically tied to the arguments being constructed. We have also added material regarding the systemic approach we adopt to make this point clearer, and we have significantly restructured section 2 to bring key messages to the forefront. Please see our specific comments below.

Comment 2c: In specific terms, clarify "closure relationships". Are (8a,b,c) the "closure functions"?

Response: The term "closure relationships", as used by the authors in related environmental modelling fields, is intended to refer to the formalisation of certain contextually-specific relationships with mathematical functions in order to fully resolve interdependencies required to make equations determinate. This requirement arises due to there being more unknown variables than equations, thus the closure relationships are needed that relate one unknown to another in order to cut down on the unknowns and resolve the equation set. We have clarified this definition with a statement to this effect in the second paragraph of section 2.2. Additionally, we have used the term "closure relationships" consistently throughout and deleted references to "closure functions" so as to alleviate confusion in this regard. We have also included an example of how such relationships might be resolved under different circumstances following Eq. 8 in section 3.6. Finally, we have specified in detail in Table 1 how each of the closure relationships has been resolved in the stylised model for each case study context.

Comment 2d: I am confused by your use of drivers and forcings. See comment at the top of p. 636 in the manuscript, By "drivers of human forcings" do you mean the rationale for (an explanatory conceptualization of) *why* humans do what they do?"

Response: The Reviewer's interpretation is correct. The term "forcings" was intended to refer to the boundary conditions or scenario-based analyses that have traditionally been imposed on the hydrological cycle to understand the impacts thereof (i.e. changes in land use, extractions and storage infrastructure). However, in the coupled co-evolving sense, the "forcing" becomes the "feedback". We have revised this terminology in the second paragraph of section 2 so as to alleviate any confusion. We have done so in the first instance by amending "the impacts of human forcings" to read "the impacts of land and water management decisions". Similarly, we have amended the sentence which previously read "the drivers of human forcings" to currently read "the drivers of the human feedback component". We believe these revisions more accurately communicate our intention.

Comment 2e: Important points are made, though almost lost, starting all the way down in Section 2.3. Explanation of Figs. 1 and 3 should, in my view, come much earlier in the paper.

Response: We have made substantial revisions to this section as follows:

- We have expanded on the "systems" approach we are adopting in the proposed conceptual framework in the third paragraph under section 2.
- We have brought "The two key feedback loops" forward to section 2.1 (previously section 2.3) to precede the detailed justification of the Sensitivity variable, and we have introduced Fig. 1 at the beginning of this section. This therefore highlights the key theories underlying our proposed framework.
- We have introduced a revised Fig. 2 in the final paragraph of section 2.1 and included a detailed description of the hypothetical trajectory of the sensitivity variable. This has been done with the intention of highlighting what we believe is a key feature of this framework, and to make the following sections easier to follow.
- Section 2.2 then lays out a more concise conceptual foundation underpinning the Sensitivity state variable.
- We have deleted the vast majority of "Converging approaches in the literature" (previously section 2.2) and moved 3 key points from within that section to the remainder of section 2.

We believe this revised structure of section 2 communicates the key messages more effectively and enhances the readability of the manuscript.

With respect to Fig. 3, given its complexity the authors feel that it needs to be associated with the detailed description of the model framework in section 3, and we have therefore left its introduction at the beginning of section 3 as it was previously. We feel that it is important that the figure be preceded by the detailed discussion of feedback loops and conceptual foundations central to its construction.

Comment 2f: The hypothetical trajectories in Figs. 2, 4, and 5 warrant further description. How were they derived? How might these dynamics be explained with reference to the three central parameters? Are there threshold dynamics at play? Might these be anticipated in some adaptive water management approach? Too much is left to the reader's guesswork for these to have their desired impact.

Response: We have substantially revised Fig. 2 such that it is currently a completely idealised hypothetical diagram that is not tied to any particular case study (as was the case in the previous version when it was linked to the Murrumbidgee catchment). A detailed explanation of Fig. 2(a-b) has been added as a final paragraph in section 2.1. Furthermore, an explanation of the influence of the macro-scale parameters appearing in Fig. 2(c) has been added to section 3.5 (second last paragraph) such that it follows the detailed discussion of these parameters.

We have also expanded on the caption in Fig. 4 to further explain the dynamic variables included in the simple water balance bucket model. Finally, we have added the results of the stylised model for both case studies, presented in section 4.3, Fig. 6 and 7, and Table 1. We believe that the results presented in Fig. 6 and 7 aid in providing further context, explanation and support to the hypothetical diagrams in Fig. 2 and 5. Table 1 has also been substantially revised such that the links between the equations, hypothetical trajectories and results are made more explicit.

Comment 2g: Groundwater appears not to enter the storage term in the two case study catchments, yet we know globally that this is a resource of rapidly expanding importance. Can the generic relevance of the proposed conceptual model be improved to account for groundwater socio-hydrological dynamics?

Response: The framework we propose is general, and it is our intention that the way it will manifest in any particular site will depend upon local environmental conditions, such as whether surface or groundwater is exploited by humans and for what purpose. We concur that groundwater is an increasingly important element of the socio-hydrology investigation, which is why we account for groundwater storage levels (S_{GW}) and extractions (R_{GW}) as part of the generic catchment water balance model we included (see section 3.1 and Fig. 4). It so happens that the two case studies used here are specific, and groundwater extractions are a minor component within these catchments. However, the model presently incorporates the functionality required to appropriately account for more significant groundwater depletion in sites where it is a more crucial component of the cycle. We have clarified this point at the end of the first paragraph in section 4.3, and by expanding on the storage and extraction variable definitions included in the caption of Fig. 4.

MANUSCRIPT COMMENTS:

Comment 3: p.630 *Line 9* Abstract should clarify sensitivity and behavioural response to what?

Response: As outlined in the "Specific Comments", we have made these clarifications in the revision. Both the Sensitivity and Behavioural Response variables are defined in more detail in the revised abstract.

Comment 4: p.630 *Line 19* Hydrologists are already participating. Perhaps "allow hydrologists to improve SES modelling through better representation of human feedbacks on hydrological processes" (?)

Response: We thank the Reviewer for this suggestion. We have revised the wording of this last sentence in the abstract and adopted the suggested wording provided by the Reviewer.

Comment 5: p.632 *Line 18* This transition is rather abrupt. Indeed, I don't find that the preceding paragraph on IWRM adds to your argument. Either show, in very specific terms, what IWRM adds to coupled systems understanding as well as its limitations (that your proposed approach will address, again in specific terms), or delete this IWRM section.

Response: We concur with the Reviewer's comment. We have therefore substantially revised the way in which IWRM is mentioned in the manuscript. We have deleted this original lengthy paragraph and instead added two sentences in the following paragraph noting that IWRM was historically the favoured research directive and highlighting the shortcomings of this approach from a systems perspective. We have cited Sivapalan et al. (2012) and Liu et al. (2008) to underscore this point.

Comment 6: p.634 *Line 2* Indeed, when it is applied to the hydrological cycle, IWRM becomes Integrated River Basin Management. But in its broadest form, IWRM attempts to address non-water *sectors* such as energy, food, etc.

Response: We thank the Reviewer for this comment. We would like to note that, in line with the revisions made with respect to IWRM (see Comment 5 above) we have deleted all further references to IWRM throughout the remainder of the manuscript, electing instead to focus on the coupled system inquiry at hand.

Comment 7: p.635 *Line 4* This is a useful "roadmap" paragraph.

Response: Noted.

Comment 8: p.635 *Line 17* These do not adequately capture exogenous and endogenous drivers. Please spell this out further. Are internal marginal changes only the product of "hydrological signatures"?

Response: This statement was intended to communicate that changes in dynamics of the catchment system may be driven by exogenous factors external to the catchment system (such as climate change, changes in market prices or global demand for commodities, political changes) or endogenous factors generated by the internal feedbacks and co-evolutionary dynamics within the catchment system (as stipulated in the assumptions and component equations of the model framework). We have clarified this sentence accordingly in the revised manuscript.

Comment 9: p.636 *Line 1* This is confusing use of "drivers" and "forcings". By "drivers of human forcings" do you mean the rationale for (an explanatory conceptualization of) *why* humans do what they do?

Response: Please see our response to Comment 2d. We have revised this sentence by using the term “feedback component” rather than “forcing” to more effectively communicate our intention.

Comment 10: p.636 *Line 16* This is helpful. Can you clarify briefly what is meant by "catchment states"?

Response: We refer to a “catchment state” as a unique combination of “state-space” variable values. For example, a 3D graph composed of three state variables, such as water availability (S_T), degraded land (A_D) and catchment GDP (E_C) on each axis, forms the matrix of potential catchment states. A catchment may thus move from state 1 at a point of “high S_T –low A_D –low E_C“ to state 2 of “low S_T –high A_D –high E_C“. Negative feedbacks will stabilise the system such that it will resist being pushed away from its original position, however positive feedbacks and unstable dynamics will induce a shift to the second position. We have added this clarification and example in the revised manuscript.

Comment 11: p.638 *Line 13* See my point at the top of p. 636.

Response: This sentence has been deleted as part of the revision of section 2.

Comment 12: p.639 *Line 27* Do you mean subjective? I could conceive of human response to perceived threats to quality of life that are not emotive but based on a clear logic, although this certainly varies by individual and by case.

Response: We have substituted the word "subjective" in place of "emotive" as it more appropriately conveys our intent.

Comment 13: p.640 Section 2.2 is a rather lengthy exposition of concepts that are only loosely tied to your proposed approach. Consider deleting this section entirely.

Response: As outlined earlier, sections 1-2.2 (inclusive) have been substantially reorganised and revised to be more concise and targeted. We have essentially deleted the vast majority of what was previously section 2.2, with three notable exceptions:

- The point that was previously made on p.641 lines 19-29 regarding the applicability of an exposure-sensitivity-resilience approach in a coupled human-environment context has been retained and moved to within the discussion of the second feedback loop in section 2.1 as it lends support to the way our framework is constructed;
- The sentence previously on p.640 lines 24-27 regarding the application of resilience theory to coupled human-nature systems has been moved to section 2.2; and
- The two sentences previously on p.642 (lines 3-5 and 14-19) have been retained in section 2.2 as they reference the current limitations and scope for future enhancements inherent in the construction of the Sensitivity variable.

We have therefore woven these specific points into the narrative in a more tailored way as part of our revision to help underscore certain arguments.

Comment 14: p.642 *Line 13* This may be problematic, for reasons including measurement. Indeed, individuals and communities respond in numerous, diverse ways to environmental change. Do I understand your statement here to refer to identifiable and sustained collective action as a response?

Response: We have deleted this point in the revised manuscript. The inclusion of this point initially was more to highlight other areas of research pertaining to the use of community perceptions in relation to Natural Resource Management, however in the interests of conciseness we did not feel it was a crucial point to make. We note that support for the idea of collective action as a response is presented in section 2.1 (5^{th} paragraph).

Comment 15: p.643 *Line 13* Vague wording, particularly when lines 8-10 above refer to demand. Indeed, many regions have effectively decoupled socio-economic development from aggregate water demand.

Response: The authors acknowledge that these two sentences were not very clearly worded. We have revised them to more effectively communicate our meaning: "as a rural catchment with a predominantly agricultural micro-economy increases in prosperity, water demand will originate from additional sources independent of population growth to a point, e.g. from the manufacturing sector, thermoelectric sector and increasingly sophisticated domestic household needs (as observed by Flörke et al., 2013)". Whilst we agree that many urban centres do experience a general disconnection of economic activity and aggregate water demand, we highlight that the framework as it is presented here is not intended to cover this complexity.

Comment 16: p.643 *Line 25* Efficiency may have the opposite effect, of increasing flows; see our paper in this HESS special issue, Scott C.A., S. Vicuña, I. Blanco-Gutiérrez, F. Meza, C. Varela-Ortega. In review. Irrigation efficiency and water-policy implications for river-basin resilience. Hydrology and Earth System Sciences.

Response: We acknowledge that this sentence was liable to cause some confusion given the existence of the "efficiency paradox" observed by Scott et al. (2013). We have therefore revised the sentence to delete reference to efficiency measures, such that it reads: "to the extent that water flows reduce, water quality deteriorates or land degrades, economic growth will naturally be constrained", as that is the essence of the point we are making in this context.

The authors would also like to note that we believe both these potential efficiency impacts are captured by Equation (7b). To the extent efficiency improvements are made, the positive impacts thereof (i.e. water savings) would reduce the demand component, D_E, via the efficiency improvement term, ζ. Any adverse efficiency impacts as a result of expansion of agricultural land on account of water savings are intended to be reflected in the first component of the equation, $\left[\frac{\Delta P_n}{P_n^t} + f(Z_C)\right]$, which seeks to capture demand for agricultural expansion within the catchment.

Comment 17: p.645 *Line 17* Excellent, I suggest using this level of specificity earlier in the manuscript including the abstract.

Response: We thank the Reviewer for this suggestion. We have incorporated it in our revision of the abstract such that the point is made upfront.

Comment 18: p.646 *Line 24* Does this account for groundwater (storage), which has been shown to exhibit particularly strong socioeconomic-hydrological feedbacks in a climate-change context, e.g., see past work including Scott, C.A. 2013. Electricity for groundwater use: constraints and opportunities for adaptive response to climate change. Environmental Research Letters 8 (2013) 035005, doi: 10.1088/1748-9326/8/3/035005.

Response: We concur that groundwater is an important element of the socio-hydrology investigation, which is why we consider groundwater storage levels (S_{GW}) as part of the catchment water balance model we propose (see Lines 11 and 23 on this page, as well as Fig. 4). As used in our paper (and defined in Table A1), S_{max} refers to total man-made water storage capacity within the catchment. We acknowledge that total water stored in such man-made structures, S_Q, will be derived from different sources depending on the catchment context (i.e. re-routing of river flows or groundwater pumping and subsequent storage) and leave it to individual practitioners to formulate a catchment model that captures the important catchment-specific considerations. We have revised the caption to Fig. 4 to make it clearer that all stores are intended to be accounted for.

Comment 19: p.648 *Line 4* Given the emphasis on rationales (drivers?) for human forcings of hydrological processes, it would make sense to separate these so that attribution of water supply can be assessed.

Response: We concur with this statement, and we have indeed considered these components separately in Eq. (2a). We have revised the opening sentence to alleviate any confusion: "Within the model framework the economics of the catchment, captured in its simplest form, can be made up of a benefit component (i.e. land productivity) and a cost component (i.e. broken down into agricultural cost and water supply cost)."

Comment 20: p.649 Eq. (2a):

- (i) Bc is crop or biomass productivity per unit area.
- **Response:** This observation is correct.
- (ii) In practice, tau A is very difficult to estimate, particularly multiplier effects over multiple years.
- **Response:** We acknowledge that agricultural growth multipliers are difficult to estimate with accuracy and a complex calculation is beyond the scope and intent of this paper. However, it would be possible to incorporate a simplistic calculation at this stage comprised of the annual national households saving rate. We have inserted a sentence to this effect at the end of the paragraph preceding Eq. 2(a).
- (iii) Is water priced/charged volumetrically in Australia or by land area proxy, number of irrigation turns? p_w will vary by irrigation and household use.
- **Response:** In Australia water is charged volumetrically. We thank the Reviewer for this second point and concur that it is important that this variability in the price of water be made explicit in the equation. We have amended the equation to include two distinct water prices – p_{wc} and p_{wp} – to apply to irrigation and household use, respectively.
- (iv) How does E_{ext} account for remittances from outside the catchment?
- **Response:** Our intention in building in this flexibility is to demonstrate that income generated within the catchment from non-agricultural sources may be dealt with in a number of ways. For example, in the event that a catchment community also has a fishing industry component, the income generated from this industry could be captured in one of two ways. The first is through a dynamic model or equation similar to Eq. (2a) tailored to the fishing industry which generates a net benefit to the catchment community. Alternatively, fishing industry profits could more simply be treated as a boundary condition and incorporated via E_{ext} (i.e. dollar per annum metric derived from fishing activity). We leave it to individual practitioners to determine which approach is more appropriate depending on the nature of the investigation being undertaken, and we highlight the opportunity this presents for the model framework to couple with more complex socio-economic models. We have substantially revised the wording in the final paragraph in section 3.3 to make these possibilities clearer to the reader.
- (v) Perhaps more importantly, how are agricultural subsidies accounted for (not through p_c, which you have noted are global prices). I refer to crop payments and insurance, conservation easements, etc.
- **Response:** We thank the Reviewer for this observation. The authors believe that the treatment of subsidies will most appropriately be dealt with within the agricultural cost component. However, given the diverse forms that subsidies may take, we have intentionally left it to individual practitioners to determine the best catchment-specific

approach given the nature of the subsidy. We have added a sentence explaining these intentions at the end of the paragraph following Eq. 2(b).

Comment 21: p.651 *Line 3* Comment on how this might be done, e.g., user defined through stakeholder engagement?

Response: We are grateful for this suggestion. The Reviewer's example is in line with what we are implying, and with the stakeholder survey technique proposed in the cited study: Imberger et al 2007[*] (Line 4). We have inserted a statement in brackets to further clarify this.

[*] Imberger, J., Mamouni, E. A. D., Anderson, J., Ng, M. L.., Nicol, S., Veale, A.: The Index of Sustainable Functionality: A new adaptive, multicriteria measurement of sustainability - Application to Western Australia, Int. J. Environ. Sust. Dev., 6, 323-355, 2007.

Comment 22: p.652 *Line 9* This is a major, and in my view unsubstantiated, assumption. Many catchments in industrialized societies are stuck in "rigidity traps" with low resilience.

Response: The authors thank the Reviewer for this comment and we acknowledge the existence of rigidity and lock-in traps that can prevent innovation and adaptation in some developed societies (Scheffer and Westley, 2007[#]). The authors would like to note however that, within the context of this paper, we are specifically referring to resilience levels with respect to stress to the hydrological cycle and impacts thereof, and a nation's *ability* to adapt and respond to such stress. The studies cited in the subsequent sentences (lines 11-18) have explained the link between national socioeconomic development and resilience levels by virtue of the degree of economic diversification and technological capacity. Within our model framework, we would argue there is sufficient evidence to support the general hypothesis that wealthier more developed economies are more *able* to proactively respond to water stress by modifying the catchment water balance, thus making such societies less sensitive to these pressures. This does not in itself imply that the society will in fact implement such changes, but rather that it has the ability to do so. In this way, we seek to capture how the macro-scale socioeconomic parameter, β, interacts with the Sensitivity variable, V. We have amended this sentence to highlight that it is "perceived" resilience that is expected to increase (i.e. community sensitivity is expected to decrease). By way of further clarification, we have added two sentences to the paragraph that highlight the distinction we are drawing between a society's perceived ability (and hence its impact on perceived sensitivity), as opposed to its actual behaviour, and we explicitly note the existence of rigidity and lock-in traps.

[#] Scheffer, M., and Westley, F. R.: The evolutionary basis of rigidity: locks in cells, minds, and society. Ecol. Soc., 12, 36-48, 2007.

Comment 23: p.652 *Line 19* In practice it is difficult to estimate HDI at the catchment scale.

Response: By way of clarification, the aim of the three "macro-scale contextual parameters" is to set the regional and national context of the catchment, so that we can ultimately compare case studies across gradients of climate, socioeconomic development and political regimes. In this way, the climate regime is intended to reflect regional climate/ aridity within which the catchment is located, whilst the socioeconomic (HDI) and political (CPI) parameters are intended to reflect the national context within which the catchment is located. We have made this approach clearer through the addition of wording in the second paragraph that clarifies that the "national or regional scale" elements are referred to as "macro-scale contextual parameters", and an elaboration on what those parameters are within the context of this framework. The HDI is therefore used to set the *national* socioeconomic context, whilst catchment-specific economics are captured in the economic return component of Eq. (4). We have revised the wording in the opening two sentences of this paragraph to make it clearer that β is designed to operate at a national level.

Comment 24: p.653 *Line 19* It would be helpful to succinctly state here that, for lack of additional data, the proxies to be used for climate, socioeconomic, and political parameters are dryness/aridity indices, HDI, and CPI, respectively. (It's not clear to me how you will estimate the latter two at catchment scale).

Response: We have incorporated this suggestion in the revised manuscript.

Comment 25: p.655 *Line 15* This makes sense in principle; I'll see how you actually parameterize and quantify these.

Response: We would like to refer the Editor to the revisions made to Table 1 where an approach to parameterisation of these normalised variables is discussed for application of the stylised model to the case studies.

Comment 26: p.658 (6a) and (6b) appear to be primarily for shape fitting.

Response: The Reviewer is correct in his observation that Eq. (6a) is primarily for shape fitting. However, the authors would note that Eq. (6b) is based on the premise that, in considering a change in the Sensitivity variable, it is important to consider both (i) the relative magnitude of the change (i.e. whether the change represents a 5% increase in sensitivity levels or a 50% increase), as well as (ii) the baseline value of the Sensitivity state variable along the scale (i.e. relative to the maximum, V_{max}). This hypothesis is explained in lines 12-19 on p.657.

Comment 27: p.659 *Line 21* Difficult to assess if extra-local demand, e.g., virtual water, is accounted for.

Response: The Reviewer makes an excellent point. Virtual water trade will no doubt form an essential component of future iterations of socio-hydrology models, as further complexity is built in and our understanding increases. However, at this stage, consideration of virtual water trade is beyond the scope of this paper.

Comment 28: p.661 *Line 25* What about groundwater? I see below and for the Toolibin that groundwater levels are rising, causing soil salinisation. I think a generic approach would need to account for groundwater depletion, perhaps a more ubiquitous challenge globally than the one faced in your two catchments.

Response: The authors concur that groundwater pumping for irrigation is more widely used in other parts of the world. Groundwater extractions do not presently form a significant component of either of the case studies presented. Nevertheless, as noted in our earlier comments, the catchment model we propose does indeed include groundwater stores (S_{GW}) and accounts for groundwater extractions (see section 3.1 and Fig. 4) in the examination of the co-evolutionary dynamics of the catchment. Thus the model incorporates the functionality required to appropriately account for more significant groundwater depletion in sites where it is a more crucial component of the cycle. We have added two sentences at the end of the first paragraph in section 4.3 to highlight this point.

Comment 29: p.663 *Line 28* Tipping point or threshold behaviour?

Response: The changes observed in the Lake Toolibin catchment to date are evidence of threshold behaviour (salinisation is a positive feedback process).

Comment 30: p.664 *Line 15* Within somewhat narrow parameters, yes, they are distinct. Do they capture a wider range of conditions, however, that might be encountered in other parts of the 'three-parameter' universe, i.e., different configurations of climate, socioeconomic, and political conditions?

Response: We thank the Reviewer for this observation. These two cases are located in Australia (hence will have the same socioeconomic and political scores) and both have similar regional climate regimes. Thus the macro-scale parameters will effectively be held constant. Our main aim in introducing these case studies was to allow readers to get a practical view for how "real-world" problems can be translated into our mathematical framework – we concede that a full attempt to explore the utility of the framework requires a major initiative. We have therefore deleted reference to the "distinctness" of the case studies in section 4. Following the preliminary stylised model results, our intention is to apply the full model to these case studies in order to assess its robustness in real life scenarios. The authors' ultimate aim is to apply this framework, and see it applied by others, across a diverse range of case studies falling within the 3 gradients (i.e. across climate, socioeconomic and political gradients) – it is only through wide-ranging comparative studies that universal patterns and principals will begin to present themselves and advance our knowledge of how feedbacks work at a system scale.

Comment 31: p.665 *Line 26* Yes, hydrologists are placing new emphasis on socio-hydrology, but the field encompasses many other disciplines.

Response: We note that this sentence has been deleted in the revised manuscript.

Comment 32: p.680 Table 1 – This is helpful for explanatory, illustrative purposes.

Response: We thank the Reviewer for this comment and note that Table 1 has been substantially revised to include greater detail as to assumptions, numerical parameterisations and the definition of closure relationships for each case study, in addition to data sources used to construct the stylised models.

Comment 33: p.687 Figure 3 – See my comments about a) remittances and b) subsidies and support programs, both of which have been shown to influence catchment resilience.

Response: We have amended Fig. 3 such that a "subsidies" parameter bubble is included to highlight its importance.

The authors are grateful for other minor suggestions throughout the manuscript and have included these amendments in the revised version.

2. Review comments by Anonymous Referee #2.

General Comments:

Comment 1: Overall, I found the review of social-ecosystem interactions to be impressive, well written and a welcome addition to that of the resilience and social-ecological-systems (SES) literature; which too often overlooks much of the material presented.

Response: Thank you for this comment.

Comment 2: However, I feel it is very long and at the expense of adequate attention being given to the quantitative significance of viewing ecosystems as coupled to society. For many readers this point would be missed and, hence, the perceived significance of the manuscript would be diminished. I urge the authors to present some clear examples.

Response: As stated above in relation to Assoc. Prof. Chris Scott's comments, we have substantially revised and restructured sections 1-2. In doing so, we have attempted to be more concise in the construction of arguments presented. With respect to quantifying the coupled system, we have taken the additional step of developing a stylised ("toy") model with idealised assumptions and simplified parameterisations to illustrate the numerical application of the conceptual framework to the two case studies discussed. We have substantially revised Table 1 to provide detail as to these assumptions and parameterisations. We acknowledge the results presented in Fig. 6 and 7 are based on idealised cases, and the application of the a fully parameterised and coupled model to these cases will likely yield much deeper insights, however we feel these preliminary findings enable a demonstration of how the theoretical framework could be translated to a numerical case. We would like to highlight that the main aims of this manuscript are to present a framework for socio-hydrologic modelling, to introduce Community Sensitivity as a key state variable, and to provide a demonstration as to how the framework could be parameterised to capture the socio-hydrologic dynamics in two different examples.

Comment 3: Additionally, the focus of the manuscript appears to be coupled positive feedbacks and nonlinear thresholds (as inferred by the model structure being based on a positive feedback, see Fig. 1). However, inappropriate depth has been given to this field of research. There was no discussion of the types of hydrological or societal positive feedbacks that have been proposed (or observed), their likely prevalence within hydrological systems, the phenomena that positive feedbacks may only arise when ecosystems and society are coupled and alternative approaches for modeling such coupling (e.g. game theory, continuation analysis) and how the proposed framework fits in with these methods.

Response: Following major revisions, we feel that section 2 provides a more logical detailed basis and justification for the feedback loops discussed. We have added a discussion of the systemic approach we are adopting in the third paragraph under section 2 to make our intentions clearer. Section 2.1 in particular provides a great deal of field evidence of the positive and negative feedbacks that form the premise of our framework. We believe that the restructuring of arguments in section 2 has made these points clearer and we acknowledge that many points may have been lost in the lengthy exposition of concepts included throughout this section in the previous version. We have also substantially revised Fig. 2 and provided a detailed explanation of the expected feedbacks occurring during the various phases of the system cycle. We would like to note that additional evidence is cited throughout sections 3.5-3.6 with particular relevance to how the feedback loops manifest in the individual functional components. We have sought to provide this foundation by highlighting discoveries and developments in diverse fields of research including hydrology, environmental modelling, sustainability science, social sciences, economics and psychology. Finally, we have added specific case study examples where the hypothesised trajectories of positive and negative feedback loops have been observed in practice globally in coupled socio-hydrology systems (see the final paragraph in section 2.1) to lend support to the theories outlined in the manuscript.

Comment 4: Lastly, considering the manuscript is proposing a hydrological coupled approach, there is an insufficient attention given to what, if anything, makes the study of hydrological systems different to other coupled investigations. For example, does the continuous disturbance from climate cause any challenges?

Response: We thank the Reviewer for this comment. In relation to the first point, we concede the importance of articulating the distinction between the specific hydrology context presented here and other coupled investigations of socio-ecology. This is especially pertinent to the unique challenges faced in investigations of models of catchment systems. Whilst we appreciate many similarities between other models of socio-ecology in general terms, the specific issues faced at the river basin scale include the potential for large scale hydrologic infrastructure development (dams and river regulation) and links between water availability and water quality that we felt deserved a more hydrologist-centric framework. Furthermore, the resolution of the slow processes that characterise the hydrological system, non-stationarity in climate, and long timescales required to monitor threshold shifts are all distinct features of such investigations. With respect to the final point regarding climate change, we acknowledge the importance of highlighting the specific vulnerability and responsiveness that the hydrological coupled system would display in this regard (e.g. Ribeiro Neto et al 2014*). We have added wording in section 1 (p.5) to highlight these points.

* Ribeiro Neto, A., Scott, C. A., Lima, E. A., Montenegro, S. M. G. L., & Cirilo, J. A.: Infrastructure sufficiency in meeting water demand under climate-induced socio-hydrological transition in the urbanizing Capibaribe River Basin–Brazil. Hydrology and Earth System Sciences Discussions, 11(3), 2795-2824, 2014.

Comment 5: The manuscript introduction contrasts Integrated Water Resource Management with SES and, while not clearly stated, the focus of the manuscript appears to be SES (please make this clear).

Response: The authors' initial intention was to contrast the various approaches that have been employed with respect to human-nature system interactions. The manuscript does indeed adhere to an SES approach (where the systems are viewed as intrinsically coupled and co-evolving). To alleviate any confusion we have substantially revised the way in which IWRM is mentioned in the manuscript, so as not to detract from the coupled-system focus of the manuscript. We have deleted much of the original paragraph discussing the paradigm and instead added two sentences in the following paragraph noting that IWRM was historically the favoured research directive and highlighting the shortcomings of this approach from a systems perspective.

Comment 6: To date, SES modeling has often used relatively simple models that allow the exploration of specific social-ecological interactions (often using nonlinear dynamics techniques). However, in this manuscript a relatively complex model is proposed for exploring a wide range of social-hydrological interactions. So, while the concept of a sensitivity state variable was interesting (and to my knowledge, novel), I am not convinced that a relatively complex model could be applied to produce meaningful insights into social-hydrological coupling by way of either numerical calibration and simulation or be amenable to application of various SES and resilience techniques for exploring the feedbacks, thresholds and steady states.

Response: The authors have drawn on recent concepts and findings in the system dynamics and SES literature (e.g. citing Lade et al 2013, Schlüter et al 2012[1]) where idealised models relevant to SES issues have been used to explore theoretical state-space relationships and response trajectories. We have specifically sought in this framework to find a compromise between theoretical relationships and "real world" dynamics and challenges, in answer to the call for "use-inspired hydrologic science" made by Thompson et al (2013)[2]. We believe the preliminary results of the stylised ("toy") model for the two case studies now presented in Fig. 6 and 7 are encouraging.

We acknowledge that full numerical calibration of a model built in line with this framework will be a challenging undertaking and cannot be achieved by application to a single or even several case studies. However, we believe that over time sufficient case study examples will emerge which could cover a range of gradients, and slowly provide confidence in the more complex parameterisations. Further, the model framework is presented in completeness to provide a larger vision and guide to hydrology modellers, but when it comes to specific implementations, aspects of the model can be simplified or removed from the key components (as has been done in the stylised examples presented – refer to Table 1) to make it more tractable or subject to systems analysis techniques. We have added a paragraph acknowledging these points in section 4.4. Indeed, a number of recent papers already appearing in the special issue have presented models that follow broadly similar approaches as what is being proposed in our manuscript (Di Baldassarre et al 2013a, 2013b, Liu et al 2013, Pande et al 2013, van Emmerik et al 2013 (submitted))[3], demonstrating that the parameterisation and implementation of such models in real systems is achievable.

[1] Lade, S. J., Tavoni, A., Levin, S. A., and Schlüter, M.: Regime shifts in a social-ecological system, Theor. Ecol., 6, 359-372, 2013.

Schlüter, M., McAllister, R. R. J., Arlinghaus, R., Bunnefeld, N., Eisenack, K., Hoelker, F., Milner-Gulland, E. J., Müller, B., Nicholson, E., Quaas, K., and Stöven, M.: New horizons for managing the environment: A review of coupled social-ecological systems modeling, Nat. Resour. Model., 25, 219–272, 2012.

[2] Thompson, S. E., Sivapalan, M., Harman, C. J., Srinivasan, V., Hipsey, M. R., Reed, P., Montanari, A., and Blöschl, G.: Developing predictive insight into changing water systems: use-inspired hydrologic science for the Anthropocene, Hydrol. Earth Syst. Sci., 17, 5013-5039, 2013.

[3] Di Baldassarre, G., Kooy, M., Kemerink, J. S., and Brandimarte, L.: Towards understanding the dynamic behaviour of floodplains as human-water systems, Hydrol. Earth Syst. Sci., 17, 3235-3244, 2013a.

Di Baldassarre, G., Viglione, A., Carr, G., Kuil, L., Salinas, J. L., and Blöschl, G.: Socio-hydrology: conceptualising human-flood interactions, Hydrol. Earth Syst. Sci., 17, 3295-3303, 2013b.

Liu, Y., Tian, F., Hu, H., and Sivapalan, M.: Socio-hydrologic perspectives of the co-evolution of humans and water in the Tarim River Basin, Western China: the Taiji–Tire Model, Hydrol. Earth Syst. Sci. Discuss., 10, 12753–12792, 2013.

Pande, S, Ertsen, M and Sivapalan, M.: Endogenous technological and population change under increasing water scarcity, Hydrol. Earth Syst. Sci. Discuss., 10, 13505–13537, 2013.

Van Emmerik, T. H. M., Zheng Li, Sivapalan, M., Pande, S., Kandasamy, J., Savenije, H. H. G., Chanan, A., and Vigneswaran, S.: Socio-hydrologic modeling to understand and mediate the competition for water between agriculture development and environmental health: Murrumbidgee River Basin, Australia, Hydrol. Earth Syst. Sci. Discuss., 2013 (submitted).

Comment 7: With regard to the calibration and simulation, I am skeptical that a model with so many parameters can be calibrated using annual data.

Response: The authors would like to note that research into the coupled dynamics of the socio-hydrology system is in its early stages and also refer to the previous comment. We have put forward a conceptual framework that seeks to reconcile a number of theories and findings from diverse research streams, and in doing so, to introduce novel components in the way the coupled system may be viewed and analysed. The conceptual framework is intended to help hydrologists see how routine hydrological investigations (and models) can be adapted to quantify impacts of societal interaction on the catchment water balance. In this regard it is a stepping stone, with the potential to be refined in future iterations as diverse case studies are examined globally and we acknowledge the specific nature of the parameterisations we have used may indeed evolve over time. We have added wording acknowledging these points in section 4.4.

We would also note the recent modelling approaches and real-world parameterisation of case studies cited in our response to Comment 6 (Liu et al 2013, van Emmerik et al 2013) which show the feasibility of such endeavours on a case-by-case basis. We do however acknowledge that full parameterisation of the framework presented herein will be a considerable task that will take time to fully address, as suggested by the Reviewer. However, this has also been true for other modelling frameworks in related environmental disciplines that contained numerous unknowns, and ensuing decades of research were required to make such models fully parameterised - for example the early Lotka-Volterra equations and Navier-Stokes were proposed originally with much uncertainty related to parameters and closure functions, yet now they are used (together) routinely for water quality prediction. At present, we do not have a way of capturing non-linear shifts in the socio-hydrological systems under currently available paradigms. This framework proposes a guided approach to address this gap. Finally, the authors would also like to highlight that the framework can simplify in the formulation of localised models, as not all parameters will be relevant for individual case studies. As noted throughout the manuscript, certain variables may even be imposed as boundary conditions depending on the relative importance of that particular parameter to the wider investigation (e.g. population dynamics may be included as a dynamic model, or population numbers supplied as a boundary condition).

We refer to the two case study examples presented in section 4, Table 1 and Fig. 6 and 7, as an encouraging start on the path to full parameterisation of this framework.

Comment 8: Unfortunately the manuscript does not present any demonstration of the framework to the case studies (note, the abstract is misleading in this respect). Site descriptions are presented but there is minimal reference back to the proposed framework.

Response: The authors concede that this was a shortcoming of the previous version and we have attempted to address this by including a quantitative demonstration of the framework to the two case studies by way of a stylised model. Although we acknowledge that at this stage the results presented are not fully coupled, we believe the preliminary results presented in Fig. 6 and 7 lend support to the underlying conceptual foundations of the theories presented. We have also substantially expanded on Table 1 to address the Reviewer's final point, such that specific reference is made within each case study to the equations and framework, and the parameterisation approach and resolution of closure relationships is detailed throughout. We also acknowledge that the previous title created an expectation that a fully coupled model is numerically applied to the case studies, which was never our intention. As such, we have revised the title to: "A prototype framework for models of socio-hydrology: identification of key feedback loops and parameterisation approach". Finally, we would like to highlight that we have labelled the work as a "prototype" framework to acknowledge it is a blueprint.

Comment 9: This severely weakens the paper and provides no means to evaluate the modeling framework. I appreciate that this work is ongoing but urge the authors to make some effort to address this weakness. If complete trials cannot be undertaken for the sites then I encourage the authors to guess parameters for each site and demonstrate the steady states and thresholds that may exist and the possible system trajectories that could result. If the latter was presented then I think it would significantly strengthen the argument of the manuscript and provide a basis for evaluation of the framework.

Response: In light of the Reviewer's concern we have presented preliminary results in Fig. 6 and 7, and section 4.3 such that readers are better able to assess the performance of the framework with reference to numerical results. We acknowledge the examples presented are idealised and hypothetical parameters have been chosen for illustrative purposes, however we believe that a fully parameterised and coupled application of this framework to either of the case studies would warrant a stand-alone follow-up paper. We are currently working to this end, however we feel that it is beyond the scope of the present manuscript.

With respect to the Reviewer's suggestion to demonstrate steady states and threshold behaviour that may exist, we have substantially revised the idealised hypothetical sketch of the Sensitivity state variable provided in Fig. 2 such that it is no longer tied to the trajectory of the Murrumbidgee Basin. We have also added a paragraph in section 2.1 that provides a detailed explanation of the feedback loops and threshold behaviour theorised for the coupled system. The preliminary results of the case studies lend support to this hypothesised trajectory.