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Interactive Comment

Interactive comment on "Socio-hydrologic modeling to understand and mediate the competition for water between agriculture development and environmental health: Murrumbidgee River Basin, Australia" *by* T. H. M. van Emmerik et al.

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We would like to thank Referee 3 for the comments that we are sure will contribute to further improvement of our manuscript.

General comment

As we mentioned already in the paper, and as we reiterate here, the model that we present in our paper is not claimed to be the cardinal novelty of our research, i.e., model development is not the end in itself but is the means to an end. We do not claim that our model is generic and can be directly applied to other human-water coupled systems (see Reply to Referee 2). The key goal of this paper is to show that socio-hydrologic modelling, abstracting human-water systems in terms of "stylized" models, can be used as a tool to explain or gain insights into the observed co-evolutionary dynamics of coupled human-water systems. In the case of the Murrumbidgee river basin (MRB), the modelling is used to reveal or bring out the interesting system feedbacks that brought about the "pendulum swing" observed in the MRB. We hypothesize that this understanding, when duplicated in other systems (see accompanying paper by Elshafei et al., 2013), can contribute towards the development of generic models that can be applied more universally. This is the long-term goal of our research. This was clearly stated in the manuscript, but in response to the reviewer's comments, we will reinforce this message in a revised manuscript.

1. However, in order to keep the equations as simple as possible, the dynamics of each subsystem is basically driven by a quite unrealistic deterministic equation where all the complexity is hidden in the parameters and the stochastic nature of the dynamics is neglected.

We are somewhat confused by the above statement of the reviewer.

First of all, as we mentioned in the general statement above, the main purpose of the paper is to distil the very complex socio-hydrologic system into its essential dynamics, which explains the need to keep the model simple. When you simplify such a system, including a hydrologic sub-system with its spatio-temporal heterogeneity, and a social sub-system that represents the collective behavior of hundreds of thousands of individuals, such abstractions may come across (if one's reference point is at some other scale) as unrealistic to readers. However, this is a price we have to pay if we want to develop models that are universal in nature, not just replicating in all the gory detail what happens in once place (as indicated in Kandasamy et al., 2014, such models already exist and are being used in the management of MRB).

In framing the governing equations we have indeed drawn on previous literature (see below), or on common intuition, with appropriate assumptions: in response to the criticism of this reviewer and Reviewer 2, in the revised manuscript we will reinforce the underlying assumptions and the literature support for the governing equations and constitutive relations.

In terms of model structure, our model is not dissimilar to several recently published "stylized" socio-hydrologic models by Di Baldassarre et al. (2013), Srinivasan (2013), and Elshafei et al. (2014), except that our model was designed with a particular case study in mind, associated with a particular dynamics that we wanted to replicate. We will compare our approach to the above mentioned models in the revised manuscript, to show that our parsimonious approach is fundamentally sound. To make the model governing equations determinate, we had to assume several constitutive relations: to properly estimate these one would need to perform field data collection and social surveys. To make them universally applicable, these constitutive relations must be anchored in common universally agreed-upon principles governing social behaviour. This will take time, and for this reason we chose to estimate these through calibration. We agree with the reviewer that it is the combination of governing (balance) equations and constitutive relations that contains all of the complexity of the system (this is obvious), and in the revised manuscript we will highlight this some more.

We are however unable to understand what the referee is implying by the stochastic nature of the dynamics. First of all, as we explained before, we are using the model to explain or understand the possible dynamics that contributed to the observed pendulum swing. We agree that the case study represents just one realization of a spectrum of complex behaviors that may arise, depending on the external drivers and internal socio-hydrologic process dynamics. If we want to derive general conclusions, then we could possible drive a generic coupled model with stochastic external drivers to generate the richness of regime behavior, including stochastic considerations. This kind of research is indeed foreseen in the future (an example of this is the work of Viglione et al., 2014 in the context of human-flood behavior, following upon the generic stylized model of Di Baldassarre et al., 2013), once the modeling framework has been tested and found adequate in several different basins.

2. Moreover, as the authors say, "the constitutive relationships that are used to link the governing equations are not prescribed; rather, both their functional forms and associated parameter values are obtained by calibration". Therefore I think that the work miss its more important aim that is to capture and better understand the complex relationship occurring between sub-systems.

We thank the referee for this opportunity to clarify our statement. We have used the word calibration in its broadest sense. Our approach to calibration is the adjustment of parameters based on expert judgment. Our method to obtain functional forms for various equations, a process which we also call calibration, is governed by two contrasting modeling demands. The first is the need to respect realistic relationships between various variables (for example, people migrate to seek better opportunities such as better agricultural production, irrigated area per capita at a given technological level should increase with increasing water resource availability, technological innovation/adoption should increase with basin scale production etc) that have been documented in relevant literature and the second need is to keep the formulation of these relationships into equations as simple as possible. We will incorporate the above discussion in our revised manuscript to clarify the concept.

In this context, we want to remind the reviewer that the approach we have adopted here for the constitutive relations is widely adopted in the hydrologic modeling literature (both conceptual and so-called physically based models), and the field of socio-hydrology has not reached a level of maturity and data support that we can prescribe the constitutive relations a priori.

3. Moreover I think that there is the risk of an over-parameterization of the sys-

tem and that the model proposed by the authors belong to the class of "sloppy model" [Gutenkunst, Ryan N., et al. "Extracting falsifiable predictions from sloppy models." Annals of the New York Academy of Sciences 1115.1 (2007): 203-211]. In short, a big ranges of parameters can be used to fit the same data, because sloppy combinations of parameters can vary over wide ranges without changing model behavior.

Please see our response to previous comments.

The reviewer contradicts himself, first claiming the model is too simple (see Comment 1 above), and now claiming that the model is over-parameterized. This must surely mean that we are about right in terms of model complexity.

The relationships between various variables are conceptually sound. Further these are not complex, reflecting intuition and supported by the literature. There is always the risk of over-parameterization (in spite of the parsimonious representation of a complex system). We agree that there may still be a risk of parameter trade-off or parameter equifinality but we believe that low model complexity controls the issue of equifinality to a certain extent.

We also want to draw attention to the fact that conceptual hydrological models of similar complexity still only have one observation with which to constrain the model parameters, namely runoff. However, in our case, we not only use time series of streamflow at the outlet (environmental flow), but also of irrigation area, population size and reservoir storage. In other words, the equifinality of model parameters is constrained to a much larger extent than in traditional hydrological models. The reviewer's invoking of the notion of "sloppy models" is quite extravagant: hydrologists have a long history of dealing with issues of equifinality and epistemic uncertainties, and are more aware of these issues than the reviewer acknowledges.

4. Perhaps one is fitting the existing behavior, but for the wrong reason. There is no understanding of the process underlying the systems dynamics.

We appreciate the comment of the referee. We will be happy in the revised manuscript to speculate on other possible reasons for the observed pendulum dynamics. We have provided one explanation for the dynamics, and this explanation is consistent with explanations of similar behavior in other systems in Australia and China (Elshafei et al., 2013; Liu et al., 2013; Liu et al., 2014). If the reviewer has an alternative explanation a counter to our explanation, we would like to know, and we will be happy to include this in our discussion.

5. The effort should be put in properly linking with a general theoretical framework already existing and recognized models for the five subsystems, with physical meaningful parameters that can be evaluated independently by data on each subsystems.

This paper formulates a model within an existing socio-hydrologic framework proposed by Sivapalan et al. (2012) and Sivapalan et al. (2014). It is the first to model basin scale socio-hydrology based on realistic and parsimonious models for the five subsystems. For example, the population evolution equation is conceptually similar to the migration model proposed by Fedotov et al., 2008. It assumes that agents migrate to seek better opportunities. Such a profit maximizing behavior has sound microeconomic fundamentals. Similarly, (change in) irrigated area per capita as a function of technology, available water storage and environmental awareness is a standard formulation for a production function. Endogenous growth theory supports our formulation of technology as a function of gross basin product (Hayami and Ruttan, 1970; Pande et al., 2013). Our formulation of hydrology and ecology of the basin is fundamentally sound as well. Temporal dynamics of environmental awareness is a formulation similar to Elshafei et al. (2014).

We would also like to highlight that the model is a socio-hydrological model with a balanced emphasis on society. We are unaware of social science models that have physically meaningful parameters. They often have parameters that modelers perceive to represent certain characteristics of a society. The parameters of our model also

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represent certain characteristics of a society. As the referee would also agree, the parameters corresponding to hydrology and ecology equations are physically meaningful. In the revised version, we will accommodate the societal meaningfulness of the parameters of our model and how it corresponds to the characteristics of the society in the basin.

6. Regarding stability of the ODE system, effect of the initial conditions.

We share the referee's concern regarding stability. However, studying the stability of the model is beyond the scope of this paper and can be a subject for a study on its own. We do believe this is a very important issue and we will discuss the issues regarding stability and the effect of the initial conditions of the ODE system in the revised manuscript, but with a view towards generating new ideas for research.

7. Sensitivity of the parameters and threshold choices.

Please see our response to the comments on the realism of the model. Sensitivity of initial condition, parameters and threshold choices is an important topic in itself and a topic for future research. As we say in response to Reviewer 2, we will perform a sensitivity analysis on the selected relationships, in order to ensure the robustness of the main conclusions of the paper. In particular, our model revealed the very interesting competition between the human productive force and the natural restorative force. We will test the sensitivity of the equations that are directly linked to these processes.

8. Take home message of the work is unclear.

We thank the reviewer for this valid comment. We will further elaborate upon the take home message of our work in the revised manuscript.

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