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Hydrology and Earth System Sciences Discussions

Interactive comment on "Parameterization and uncertainty in coupled ecohydrological models" *by* S. Arnold et al.

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Thank you for the productive comments and suggestions to our paper. We have addressed them as follows:

Comment: It would be helpful to include more detail on how parameters were varied between different species. The paper simply states that parameter space was constrained qualitatively (Table 1) since this is central to the paper – additional detail is warranted. In particular it would be helpful to record the values used for flood shape parameters. I assume that mean values were similar for model A and model C – but authors did not state this. If these differ significantly then some of the the difference between model A and model C such as greater CV in transpiration with model C could

be attributed to an overall difference in system sensitivity to flooding, rather than the effect of differences between species. In other words – you could get the same increase in CV found for model C, by shifting parameter distribution with model A.

Response: We are not sure, if we understand the comment completely. The parameters for the flood generator were the same for Models A, B and C, for each experiment. We only changed the Hurst Exponent (where indicated) in order to analyse the influence of a changed flood regime – but also this was done for Model A and C at the same time. Admittedly, we never use the same flood time series twice, since the flood generator is stochastic. We checked however, if our results are reproducible (i.e. Do 500 floods lead to the same result as 5 x 100 floods?) and they are.

Comment: I also note that in equation (10), and (7a), the function used should be min rather than max.

Response: Thank you for catching this! It was a typo, the actual model implementation uses min. We corrected it.

Comment: I also think that equation (23) should include a –Wgi(t)*Ri(ti-1) term (and then conversely in equation 26 there should be a –Wri*Gi(t) term. This would make sense in terms of maintaining a biomass balance – perhaps I am missing something but it would be helpful to clarity in the text.

Response: In our opinion, this would imply that Carbon is moved from reserve to green biomass and there would be no real growth. However, as long as a plant grows it accumulates carbon (via photosynthesis). Therefore, the overall biomass balance of a thriving plant should be positive. It is true that the term "reserve biomass" is misleading in this respect. We have tried to improve the wording in the Methods section to clarify the role of the reserve biomass: "... denotes the growing of leaves on the existing reserve biomass, assuming that the required Carbon of the reserve biomass was already accumulated in the buds during the previous season."

Comment: I disagree somewhat with the authors conclusion that mean hydrologic variables in this system are driven by hydrologic model – Note that mean transpiration varies significantly across ecological parameter values (Transpiration more than doubles across functional parameter sets) Figure 6.

Response: We agree with the point that hydrologic variables varied significantly across ecological parameter sets. However, the shape of the distribution (the ensemble statistics, Fig. 6) of the mean hydrologic variables is quite similar between model A and C. There seems to be no difference in the mean ground water table of the ensemble, for example. This is not the case for the shape of the distribution of the fluctuation (Fig 7). According to the ensemble statistics, we expect much larger fluctuations in model C than A. We clarified the relevant sections accordingly.

Also, we formulated the conclusion more carefully, by saying that we find hints that the means of the hydrological variables are probably influenced by the hydrologic system, while the fluctuations are probably driven by the ecological system.

Comment: The introduction could in some place be more clearly linked to the paper and needs some revision – For example, I found the discussion of population dynamical models versus ecohydrologic models difĩňĄcult to follow (pg 4158) and was not sure of the relevance to this paper. The authors state that population dynamical parameter summarize all relevant effects caused at the individual scale – it is unclear what effects authors are referring to. Nor is it clear what is meant by "direct" parameterization in the following sentence.

Response: We modified this section and give examples for effects caused by processes at individual scale like plant growth and mortality, response to disturbances, type and strength of competition or seed dispersal.

Comment: Line 24, pg 4158 – citation needed ("models have been. . .")

Response: We moved the citations of Laio et al. (2001) and Rodriguez-Iturbe et al.

(1999) to the sentence.

Comment: Line 29, pg 4158 – not sure what "precise order of parameter combinations" means – needs more explanation

Response: We see that this statement is not grammatically correct and therefore misleading. We erased the sentence. The context explains what we mean (we added some words):

"A given model only allows for coexistence, if its structure and parameters meet strict conditions, which provide for the required relation of trade-offs."

Comment: Line 1 pg 4159– also depends upon the structure of the model

Response: We agree with this point and modified the statement.

Comment: Line 8 – pg 4159 The statement that these studies have dealt with uncorrelated random environmental signals – needs more explanation – specific examples would be helpful.

Response: We added four citations that studied the relationship between frequency of environmental disturbances, like low ground water table or fire events, and biodiversity/coexistence of plant communities. All of these studies dealt with uncorrelated random fluctuations.

Comment: Line 27, pg 4159 "no rainfall" seems unlikely

Response: True, the right wording would be "negligible". The rainfall in this area is very rare, less than 20 mm per year (Botes et al., 2003). Also other authors state that there is no rainfall in this area (Morin et al., 2009). We have changed this.

Comment: Line 3, pg 4162 – what is similar in infiltration fluxes - eg total amount across different flood events; infiltration rate across different flood events?

Response: The infiltration rate across different flood events is similar (Dahan et al.,

2008). We changed it in the final version.

Comment: Pg 4167, line 1-4, it seems that you consider only a single flood per year. This may be appropriate given the study site but authors should include justification for this approach.

Response: In fact, there is more than one flood per year possible. However, since the model runs at a seasonal time scale, a higher resolved flood regime would neither affect the ecosystem nor the hydrosystem dynamics. Therefore, we consider the flood event to be one single event per year.

Comment: Pg 4177 line 1-4; I disagree that this paper really shows that different species can only coexist under the precondition that inter-specific competition is weaker than intra specific competition – can you elaborate – what is the measure of intra-species competition in your model?

Response: We agree with this point and modified this section.

References

Morin, E., Grodek, T., Dahan, O., Benito, G., Kulls, C., Jacoby, Y., van Langenhove, G., Seely, M., and Enzel, Y.: Flood routing and alluvial aquifer recharge along the ephemeral arid Kuiseb River, Namibia, Journal of Hydrology, 368, 262-275, 2009.

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