Interactive comment on "Parameterization and uncertainty in coupled ecohydrological models" *by* S. Arnold et al.
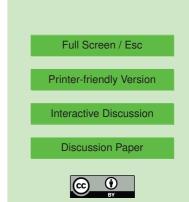
Anonymous Referee #2

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This paper presents an interesting use of an observed "pattern" – specifically the co-existence of 3 riparian plant species to constrain uncertainty in a coupled eco-hydrologic model. Uncertainty in this case is attributed to 25-30 parameter values and to 3 difference model structures (of varying complexity). While there is a broad literature on using hydrologic measurements (streamflow, tracers etc) to constrain hydrologic parameters, there are relatively few studies that incorporate ecological information to constrain model results. Thus, this paper offers an interesting example of how relatively "soft" information (the coexistence of 3 species) can be used to select "acceptable" model parameters and structures. The paper also offers insight into eco-hydrologic interactions and species competition – demonstrating both the function of niches in supporting species diversity and the role of disturbance (in this case flood).

When relatively simple mechanistic models produces functional behavior (as an emergent rather than explicitly modeled property), this allows us to better understand the factors that contribute to observed patterns, such as niche-based species diversity. This paper clearly demonstrates this utility. Overall I felt this was a good paper and of interest to both the ecological and hydrologic communities.

The methods are clearly presented and appropriate for the research questions of interest. 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. I also note that in equation (10), and (7a), the function used should be min rather than max. I assume that this is a typo and that actual model implementation uses min – however, if that is not the case then overall model would be problematic – for example, in equation 10, transpirational demand could be greater than potential evapotranspiration and transpirational demand from a very small plant would still be PET. 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. Discussion is interesting and in general well support by results. 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

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Comment

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Discussion Paper

Minor comments 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 difficult 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.

Line 24, pg 4158 – citation needed ("models have been. . .") Line 29, pg 4158 – not sure what "precise order of parameter combinations" means – needs more explanation Line 1 pg 4159– also depends upon the structure of the model Line 8 – pg 4159 The statement that these studies have dealt with uncorrelated random environmental signals – needs more explanation – specific examples would be helpful. Line 27, pg 4159 "no rainfall" seems unlikely Line 4 pg 4161 – should be "of" rather than "to" Line 3, pg 4162 – what is similar in infiltration fluxes – eg total amount across different flood events; infiltration rate across different flood events? 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. 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?

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 6, 4155, 2009.