

Interactive comment on “Propagation of soil moisture memory to runoff and evapotranspiration” by R. Orth and S. I. Seneviratne

Anonymous Referee #2

Received and published: 23 December 2012

Using a simple water balance approach, the authors investigated how soil moisture/storage and soil moisture memory affects both runoff and evapotranspiration memory in >100 catchments across Europe.

The general topic of this paper (catchment storage and its effect on various hydrologic processes) has, especially recently, received a great deal of attention from the hydrologic community and will likely stay topical in the future. While the authors present some promising results, the paper in its current form raises questions I will discuss below.

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General comments: The authors acknowledge that their “soil moisture,” a residual of the water balance, is not actually soil moisture but incorporates all sorts of storages in the catchment. Why not simply call it (catchment) storage? It would be a more accurate term for it in my opinion.

Model and model calibration: since the authors are referencing a paper in review for the methods (see technical comments), more details about model structure and especially model calibration would be desirable. What was the reason to simply use a (Pearson?) correlation coefficient and not one of the established objective functions used in hydrology (e.g. Nash-Sutcliffe)? What exactly is the accuracy term in Table 1? Also, the fact that the model actually reaches the upper limits of C_s , α , and γ suggests that the model would exceed those boundaries in order to get a better fit. Unreasonable model parameters are usually an indicator of a model structure that is not suitable for a given watershed or runoff characteristics. And it would be nice to see how the simulated Q actually compares to the measured Q (at least for the case study) since all further analysis depends on the quality of the simulated runoff. Also, how sensitive are the model parameters for the calibration, especially C_s , the only somewhat physically interpretable parameter? Simulated Q and subsequent analysis/interpretation highly depends on C_s . A non-sensitive C_s parameter would raise questions about the interpretability and significance of the results. Are the derived C_s values in a meaningful range for the chosen catchments, i.e. is there a way to compare/validate the calibrated C_s values with actual soil measurements like depth and porosity? A simple check could be plotting the C_s values for all catchments to check for consistency in catchments that are spatially close together and could be considered (somewhat) hydrologically similar.

What do Equations 4 and 6 actually mean? It looks like ξ can theoretically range from -2 to +2. What do those values mean for the coupling strength? Isn't there some equifinality in the results? For example, you can get $\xi(Q_n, w_n)=0$ for an infinite number of $\rho(Q_n, w_n)$ and $\rho(Q_n, P^*_n)$ combinations. Would the interpretation of ξ be the same, no matter what combination it is derived with? How meaningful is it to look at

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correlations between Q and w_n , and E and w_n when w_n is actually used to calculate both Q and E?

Figure 3a: Shouldn't the fitted lines have a y-maximum of 1 since the ratios are bounded by P and R_n , respectively?

How do you define “significance” in the results section? (e.g. 12116/1)

I would consider restructuring section 4. The results seem to contain methods as well, e.g. the computation of the persistence time scales in section 4.5, or the slopes of the normalized runoff and ET functions in 4.4.2. Maybe break it up into distinct “Results” and “Discussion” sections and keep methods restricted to the actual Methods section? In general, I have found section 4 to be somewhat confusing, and it's possible that some of the key findings are unintentionally obscured by the mildly confusing structure of the entire section.

What is the reason for the lack of a clear spatial pattern in the soil moisture anomalies in Figure 10? I would assume that the extent of dry anomalies exceeds the local scale and affects much greater areas. This is not evident from Figure 10.

Technical comments: 12105/20, 12106/5, 12107/20: Referencing a paper that is in review (Orth et al., 2012) is less than ideal (especially in the methods section).

Fig 5: Consistency with labels. I'd suggest using $\xi(E_n, w_n)$ instead of $\xi(\text{ET}, \text{Soil moisture})$. The same goes for runoff. The axes labels don't have units. And I would also suggest renaming the axes labels to $\rho(Q_n, W_{n+lag})$ etc, but that is just a personal preference.

Koster and Mahanama (2012) reference title wrong? Actual title: Land surface controls on Hydroclimatic means and Variability?

The nice maps (especially Figures 4 and 10) may be a little too small in their current form if the paper is printed. It's difficult to actually recognize what is going on.

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Figures 9 and 10 should be switched as Figure 10 is discussed first.

Final remark: The paper deals with an interesting topic and shows some nice results. However, referencing a paper in review in the methods section is problematic, especially when writing that the model was validated in that particular paper (12112/10). Because of that, I suggest either greatly expand the methods section and be more elaborate on the model validation or even hold off a publication until the Orth et al., 2012 paper has been published, as this appears to be a crucial paper since the current model structure is apparently being discussed there. Also, like I suggested/mentioned above, section 4 is slightly confusing (especially the “coupling” part), and I personally would prefer a clearer distinction between methods, results, and discussion.

Lastly I would suggest having a revised manuscript checked by an English editor, as parts of it are somewhat difficult to understand. To some extent the confusion from section 4 may be a result of the somewhat confusing language as well.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 12103, 2012.

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