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Hydrology and Earth System Sciences Discussions

Interactive comment on "Parameterization of bucket models for soil-vegetation-atmosphere modeling under seasonal climatic regimes" *by* N. Romano et al.

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Review of HESSD-8-5083-2011 Romano, N., M. Palladino, and G. B. Chirico Parameterization of bucket models for soil-vegetation-atmosphere modeling under seasonal climatic regimes

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General Comments: In this work, the authors examine two models of soil-moisture dynamics: a bucket-model (BM), for which the active root zone is represented as a single reservoir, and the SWAP model, a vertically discretized representation of plant-uptake and infiltration based on the Richards equation (RE). The BM requires specification of field capacity – that is the soil saturation at which gravity drainage becomes negligible – and a specific goal of the work is to examine two methods of estimating this quantity and the effect each parameterization has on the ability of the bucket model to match the RE. The focus of the work is on the parameterization of field capacity, specifically, and perhaps the title of the paper could emphasize this focus a bit more. The comparison is performed for the case of a decidedly seasonal climate, with a wet dormant season and a dry growing season. The work highlights conditions under which field capacity is an important parameter and advocates for the determination of this quantity from a flux perspective (the “drain” method) rather than from the retention curve at a fixed head value (the “fix” method).

I think the work will benefit from some additional analysis and context that offers deeper insight and explanation of the results.

Specific Comments:

1. With a focus on different parameterizations of field capacity, it makes sense to first identify conditions under which field capacity plays an important role in the water balance. For example, under consistently dry conditions, soil saturation may never get close to field capacity and the variable may not be important (that is, its specific value may not have a strong influence on model predictions of some desired quantity – soil moisture or hydrologic fluxes). For consistently wet conditions, the value of field capacity will affect model predictions of soil moisture, but may be less important with respect to partitioning precipitation to transpiration and recharge (since the conditions are always wet, transpiration demand will generally be met and the remainder of the input will go to recharge).

For seasonal conditions, field capacity plays an additional role in that it dictates the amount of water in storage at the end of the wet season and start of the dry season. I presume that this is why the authors chose to focus on a seasonal climate in this

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work. I think this concept is understated in the paper, however. That is, the manuscript could do more to point out that whether one parameterization method of field capacity is superior to another will hinge on two foundations:

a) is field capacity a relevant parameter for the soil-plant-atmosphere conditions being considered?

b) do the two methods lead to different predictions of field capacity? (in this work, they do for the loamy sand, but not really for the clay)

With respect to the first point, I think the authors can do more to explain why a seasonal climate offers conditions for which the value of field capacity plays a significant role.

2. Related to point 1 above, I suggest that the importance of field capacity and seasonality can be quantified relative to the dynamics of the system by comparing some time scales. During the dry season, the potential transpiration rate (0.46 cm/day) is four times greater than the average precipitation rate (0.116 cm/day). During the wet season, average precipitation (0.346 cm/day) is nearly double potential transpiration (0.2 cm/day). This seasonality is quite strong, and the climate is very wet during the wet season and very dry during the dry season. When the system transitions from wet to dry, the active vegetation begins to rapidly deplete the stored water in the root zone. The value of field capacity will determine (in part) the length of this depletion phase, and, thus, differences in field capacity could be significant.

Specifically, the amount (depth) of stored water in the root zone at the start of the growing season can be characterized as Sfc*n*Zr (field capacity x porosity x root depth) The plant-available water is calculated as PAW = (Sfc-Sw)*n*Zr

A characteristic time to deplete this plant-available water can then be calculated as the depth of plant-available water (PAW) divided by a characteristic rate of depletion, which could be estimated by ETmax – (mean precipitation rate).

This characteristic depletion time can then be compared to the length of the dry season

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(214 days in this case) to determine whether the role of initial storage is significant. For the two soils and two Sfc-methods explored in this work, the ratios of depletion time to dry-season length are

Loam, drain (Sfc = 0.51) = 21%

Loam, fix (Sfc = 0.67) = 30%

Clay, drain (Sfc = 0.83) = 21%

Clay, fix (Sfc = 0.79) = 19%

From this analysis of time scales, one can see that the depletion time is approximately 20% of the total dry season length. This is consistent with the small, extended tails on the pdfs of soil moisture for these conditions; i.e., for most of the dry season, the value of Sfc will not be important as the soils will be too dry.

On the other hand, moving from the dry to the wet season, one can similarly calculate a filling time: FillTime = PAW/(mean precipitation rate -ETmax) Comparing this fill time to the length of the wet season, the results are as follows:

Loam, drain (Sfc = 0.51) = 69%

Loam, fix (Sfc = 0.67) = 102%

Clay, drain (Sfc = 0.83) = 72%

Clay, fix (Sfc = 0.79) = 64%

From this simple analysis, one immediately sees that the transient filling time is nearly as long as the wet season itself, and one would expect the specific value of Sfc to therefore be more significant under these conditions (again consistent with the pdfs in Figure 5 – the rise of the bimodality can be attributed to a filling time that is long relative to the entire wet season).

Characterizing the scenarios this way lends more quantitative weight to the conclu-

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sions. The results can then be tied directly to these differences; i.e., it's not so much that the drain method is more important for the loamy sand as it is that differences in Sfc (from the fix and drain method) are much greater for the loamy sand, and the Sfc variable is particularly important during the transition from the dry to wet season.

I recommend that the authors include a quantitative analysis such as that above to improved the readers' understanding of why the results arise as they do. I think this will also strengthen the conclusions regarding the role of seasonality.

3. The authors argue in favor of the drain method of determining field capacity over the fixed method under most conditions. The fixed method they consider, however, is related to the retention curve. Would it work to consider a "fixed" method based on the unsaturated hydraulic conductivity function? For both the loamy sand and the clay, the values of field capacity determined from the drain method correspond to saturations that give values of unsaturated hydraulic conductivity equal to 0.05 cm/day. Could this be a generalizable concept to employ? That is, could field capacity be determined as the value of saturation for which K(Sfc) = 0.05 cm/day?

4. With respect to Table 5, the authors comment on the sign of the ME and a relative comparison of ME and RMSE between the "fix" and "drain" methods. Nothing is said, however, about the absolute magnitudes of these quantities. Are these values large? Small? Significant? Insignificant? Even if differences are detected between the fix and drain methods, a statement regarding the overall fit is needed to contextualize the results.

5. In relation to Figure 6, the authors state that "the bucket model outputs are not able to capture all of the variations exhibited by both transpiration and losses from leakage." I am not sure what is meant by this statement, and greater specificity would help. I also think that the mismatch in transpiration between the BMs and RE (and, therefore, in recharge) at the end of the wet season/beginning of dry season is due to the differences the transpiration function as depicted in Figure 3. That is, the RE

begins reducing transpiration before the BM, and this could account for the temporal lag in the flux curves.

Technical Corrections:

p. 5084, l. 13 sentence ends mid-stream

p. 5087, l. 13 replace “is” with “are”

p. 5089, eq. 2 What is the meaning of tau_VG? This variable does not seem to appear in any of the equations.

Figure 2 this figure is identical to figure 2 in Guswa et al., 2002; while that figure was motivated by the work of Laio et al., 2001 and IRI 1999 before that, reference should probably be made to the figure in Guswa et al., 2002.

Figure 6 I recommend that the caption indicate the differences between the left-hand-side figures and the right-hand-side. I infer that the differences might be due to the different methods of determining Sfc (fix versus drain), but it’s not clear.

References

Guswa, Andrew J., M. A. Celia, and I. Rodriguez-Iturbe, 2002. Models of soil moisture dynamics in ecohydrology: A comparative study, Water Resources Research, 38(9), 1166, doi:10.1029/2001WR000826.

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