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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.

Anonymous Referee #5

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The paper investigates how different methods for estimating the field capacity impact the results of a bucket model for long-term soil moisture dynamics. The performances of the soil moisture bucket model are evaluated in terms of the difference with a more detailed model based on the Richards equation (the SWAP model). In particular, the work considers two alternative methods to estimate the value of the soil moisture at field capacity: fix and drain method. The different methods are compared for two different soils (loamy-sand and clay) and two different seasons (dry and wet). In the case of a clay soil, fix and drain methods give very similar results for the value of soil moisture at field capacity; consequently the bucket model outputs in term of soil

moisture pdf are similar, both during the dry and the wet season. In the case of a loamy-sand soil, fix and drain methods give different results for the value of soil moisture at field capacity. During regrowth vegetation period the corresponding soil moisture pdf's differ only in the tails. Huge differences arise during the dormant vegetation period, and the determination of the field capacity threshold plays an important role. In this case, the results obtained from the bucket model using the drain method for the estimate of the field capacity are in relatively good agreement with the results obtained using the SWAP model in terms of soil moisture pdf's.

Even though the work may seem rather limited in scope, it is overall rather clear and well written, and I think it can be worth of publication in HESS. I think increasing the emphasis on the general implications of this work in terms of improved knowledge of the underlying physical processes would be in some sense beneficial, as the work much more focussed on modelling issues. Some additional comments follow.

Comments

The title refers to the parameterization of "bucket models", while a single bucket model is used. Maybe "parametrization of a bucket model for. . ." would be a better title.

The reference spatial scale of this study is unclear. Do the authors refer to the point scale? Or rather they refer to the hillslope/ catchment scale? This matter arises because, as the authors claim (p. 5085, line 25 – p. 5086, line 4), a model based on Richards equation can be applied chiefly at small spatial scales. On the other hand, larger spatial scales are more representative, and attempts to use bucket models to larger spatial scales have been done. I suggest clarifying which is the spatial scale considered in this paper.

I think the authors should clarify that during the regrowth vegetation period the threshold field capacity doesn't play a key role in the water balance because soil moisture content remains relatively far from this threshold. This should be the reason why the results obtained considering fix and drain method are similar in this case. Instead, dur-

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ing the wet season, soil moisture frequently assumes values close to the field capacity and the value of this parameter (as well as the method used to estimate it) strongly impacts the shape of the underlying soil moisture pdf. At the end, the drain method is thus preferred on the basis of a single case were it gives better results (loamy-sand soil during dormant vegetation period).

“The efficiency of simplified models has been test in areas characterized by stationary condition of the climatic forcing” (p. 5087, lines 5-6) I guess there are some numerical and analytical works aimed at extending the stationary bucket model by Laio et al., in order to take into account both seasonal and interannual variability of climate conditions [e.g., Porporato et al., 2006; Settin et al., 2007].

I guess the issue of the bimodality of soil moisture pdf is far from being new. Maybe you should further expand the ref section to include other relevant studies in this field [e.g., Kochendorfer and Ramirez, 2005; Daly et al., 2009; Vivoni et al., 2010].

Other minor comments

p. 5084, lines 8-9 (“100 time series of stochastically-generated daily rainfall data”), – see also p. 5099, line 7 and p. 5104, line 29. It is not fully clear why do you need to produce 100 (or 99) replicates instead of using a single simulation of 100/1000/. . . years? p. 5084, line 13: this sentence ends with the words “and”. Something is missing? p. 5085, line13: “Richards equations” and “offers a comprehensive” is repeated. p. 5087, line 19 and p. 5094, line 16: “evapotranpiration” reads “evapotranspiration”. p. 5089, line 13: “where θ0 and is” should read “where θ0 is”. p. 5090, line 15 and equation 2a and 2b: same parameters are identified with upper-case or lower-case letters. p. 5090, line 16:as far as I can see, the parameter τVG doesn’t seem to appear in the equation. p. 5096, lines 13-14: Should “Hortonian runoff generation” read “Dunnian runoff generation”? Figure 2: caption, replace “Evaporation and transpiration” with “Losses” – also the leakage is plotted in the graph. Figure 5: I suggest including titles in the Figure plots: “Regrowth Vegetation Period” and “Dormant Vegetation Period” for the

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two columns and “Loamy-Sand soil” and “Clay soil” for the two lines, to better disclose the differences among these plots. Figure 6: I suggest including titles in the Figure plots: “Drain method for sfc” and “Fix method for sfc” for the two columns.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 8, 5083, 2011.