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Interactive
Comment

# *Interactive comment on* "Modeling the monthly mean soil-water balance with a statistical-dynamical ecohydrology model as coupled to a two-component canopy model" *by* J. P. Kochendorfer and J. A. Ramírez

**Anonymous Referee #1**

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The topic of the paper is very interesting, and it is appropriate to HESS. However in my opinion the paper is not acceptable at the present form. It may be acceptable after major revision.

Main comments:

1) Figure 10: this figure shows that the proposed model is not able to predict soil moisture dynamics. The only test of the model is with observed soil moisture data, and the model doesn't work. Hence, in my opinion the paper cannot be published in the

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present form. The authors must improve the model. And they can use other dataset with more data available (e.g., evapotranspiration, net radiation. See fluxnet database) for testing it deeply.

2) A lot of modifications (e.g., TCA application for surface runoff, soil evaporation model) of the original Eagleson model are proposed in the paper. But the authors didn't show any improvements comparing the old and the new versions of the model. They should demonstrate that the proposed model is better than the original one.

3) The paper must be shorter. It is very difficult to follow it because it is too long. A lot of part of the paper are redundant and may be omitted. For instance: - discussions in pag. 586, 587, 602, 603, 606, 607. - Brooks and Corey equations (pag. 590), Penman Monteith equations (pag. 605), energy balance equations (pag. 607), net radiation equations (pag. 608) are old equations available in any hydrology book. Please just put references - figure 5 is not necessary. You can just leave the equations - again figure 6 is not necessary. It is an old model scheme.

Minor comments: 4) Pag. 581, row 23-24: the authors wrote that "part of that nature is also to encourage parsimonious use of parameters and driving variables". But it is in contrast with the proposed model, which includes a lot of parameters.

5) Pag. 603: what is psiu and psi_u_c I can't find any definition also in the nomenclature of symbols

6) Pag. 609, equation (75) and (76). The atmospheric stability is not included in evapotraspiration formulation, while it is widely recognized its importance in the Penman Monteith formulation (e.g. Brutsaert, 1982). The authors should justify their choice.

7) Pag. 613, row 26: Kochendorfer (2005) is not in the reference.

8) Figure 12. I supposed that the LAI was an input of the model (e.g., pag. 614), while from the figure it seems that the LAI is predicted according to the soil texture. I cant understand how and why.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 5, 579, 2008.