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Hydrology and Earth System Sciences Discussions

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 #2

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General comments

This work modifies the Eagleson (1978) model with a number of modifications, which, as the authors put it, "improve its physical realism at the expense of its mathematical elegance and analytical tractability." The modifications include introducing seasonality (dynamics at a monthly scale) to the model, incorporating the two-component Shuttleworth-Wallace (1985) canopy model, dividing the soil column into two layers with taking into account the wetting front, partitioning of evapotranspiration, and including snow as part of precipitation. The present model also excludes some components

from the original model, namely the interaction with groundwater table (i.e., no capillary rise) as well as such components as snow sublimation. The model involves numerous assumptions, many of which I cannot claim the expertise necessary to judge them; most of them seem reasonable and I trust that the authors make their choices carefully. The model could offer a new framework under which one can properly study a certain class of hydrological questions—as long as one is fully aware of the validity of those many assumptions and have decent estimates of the accompanying parameters. Indeed, the authors apply the model to real cases and offer an ecological optimality hypothesis and an explanation for the inverse texture effect. In fact, the application to real cases is the subject of another paper that the authors are also submitting to HESS (Kochendorfer and Ramirez. Ecohydrological controls on vegetation density and evapotranspiration partitioning across the climatic gradients of the central United States, submitted to HESS).

The paper is generally well-referenced, well-organized and well-written. The authors are also explicit in their assumptions and limitations of the models, both the original and their own, which should facilitate future model development. The introduction and background information throughout the paper are well-written and comprehensive. However, they are a little too tentative in sections 7 (Results and discussion) and 8 (Summary and conclusions) presumably because they try to avoid overlapping between this work and the other paper. In addition, the discussion and supporting evidence of their findings, although interesting, are not completely conclusive at some places.

All in all, I think that the paper introduces a relatively novel tool and addresses important, interesting hydrological questions certainly fitting the scope of HESS. I recommend that it be published. However, please see my comments below, which I believe could improve the manuscript.

Specific comments

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Introduction. Please state explicitly that this work is one of your two-article series.

Introducing seasonality and two-soil-layer setting. The motivation for these modifications is well explained on page 584, lines 17-24.

Two-component ET model. Inclusion of the two-component Shuttleworth-Wallace (1985) model does seem to enhance realism of the evapotranspiration process by taking into account vegetation density (page 587).

Neglecting gravity drainage in evaporation from bare soil. I think it suffices to say that the authors neglect the gravity here due to its relative size. Additional argument that"neglecting this upwards flow in the latter part of interstorm periods offsets neglecting the net flow down at the beginning of interstorm periods" seems unnecessary and, if anything, weakens the point. I suggest that the part from page 599, line 22 until the end of that paragraph be removed.

Section 6. Please include the soil texture of the R-5 watershed here.

Ecological optimality and inverse texture effect. The authors exploit the ability of the model to capture the seasonal dynamics to propose an alternative hypothesis for the ecological optimality. Specifically, they propose that the plant optimal strategy is the one in which "soil moisture in the latter half of the growing season just reaches the point at which water stress is experienced." I think that this is certainly interesting and warrants further investigation. However, at this point, the supporting evidence is not yet completely convincing. Specifically in figure 10, the model can capture relatively well only the monthly soil moisture in the root zone of the R-5 watershed and the observed root-zone soil moisture of the CPER does not go below θ_{uc}. These are presumably due to the data uncertainty; in fact, the authors admit as much: "one should not read too much into the results due to data uncertainty" (page 615, lines 8-9).

The subsequent analysis of the effects of soil texture on the peak leaf area index (LAI) offers an explanation for the inverse texture effect that rests upon the condition that the

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proposed ecological optimality mechanism is true. Therefore, I think that this explanation, too, is not yet conclusive. Also, I think that both the authors' explanation and that by Laio et al. (2001) (the last paragraph) can be valid and both could be behind the inverse texture effect.

Please do not view the previous paragraphs as negative reviews. I do think that these are interesting hypotheses that should be further tested and that are definitely worth sharing with the HESS audience. I just want to point out that at places the authors seem to present their findings as a little more conclusive that they are.

Finally, regarding vegetation optimal strategies, the authors may want to check out the works by Caylor et al. (Geophysical Research Letters 2004, Advances in Water Resources 2005).

Discussion on the slope of LAI-soil texture relationships. Precisely because of the data uncertainty, I think that the authors might be reading too much into the changes in the slopes and the discussion from page 617, line 22, to page 618, line 1, is premature and should be removed.

Figure 10. The observed soil moisture monthly dynamics of CPER and R-5 are quite different and deserve more discussion. For example, the root-zone and recharge-zone soil moistures of the CPER converge from July to October. There seems to be a time lag of a month of two between the root-zone and recharge-zone soil moisture dynamics of the R-5 watershed: the root-zone soil moisture starts to decline in March and the recharge-zone soil moisture does so in April or May, and the root-zone soil moisture starts to increase in August and the recharge-zone soil moisture does so in September. These features are not captured by the model.

Technical corrections

In the following, P stands for page and L stands for line(s).
P 584, eq 1: As this is the first equation, please mention the variable/parameter table

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at the end of the manuscript and direct the reader there.
P 586, L 1: “identically” perhaps reads ”identical”.
P 587, L 15: “does no account for” reads ”does not account for”.
P 587, L 20: “that is in” perhaps reads ”that it is in”.
P 597, L 10: Is 39 the correct equation number here?
P 603-604: R_s appears at various places where I think the authors mean R_{soil}, e.g., equation 56, P 603 L 20, and P 604 L 4; please check carefully.
P 608, L 18: “Eq. (1)” should perhaps be “(1)”.
P 608, L 21: “Eq. (2)” should perhaps be “(2)”.
P 616, L 2: “neglibility” reads “negligibility”.
P 617, L 6: “To wit”?
P 617, L 13: The phrase “Holding precipitation constant in 5-cm intervals” is unnecessary and confusing; please remove.
P 620: The second f_p should perhaps be f_s.
P 622: The s in rs_{min} should perhaps be subscript, i.e., r_{smin}.
P 627, L 10: “annu, al” reads “annual”.
P 634-635, Tables 3 and 4: E_P should read $E[P]$?

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