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Discussions

Interactive comment on "A simple ecohydrological model captures essentials of seasonal leaf dynamics in semi-arid tropical grasslands" *by* P. Choler et al.

Anonymous Referee #3

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Review of Choler. P., Sea, W., Briggs, P., Raupach, M., and Leuning, R., A simple ecohydrological model captures essentials of seasonal leaf dynamics in semi-arid tropical grasslands. Submitted for publication in Biogeosciences. BGD 6, 8661-8690, 2009

This is an interesting study that uses a nonlinear approach to model phenology in water-controlled ecosystems, coupling the dynamics of plant cover and bucket-type-modeled water balance. The study is comprehensive and incorporates long-term climatic data (8 yr) and MODIS-derived estimates of vegetation cover across a large area (400000 km2) and a large number of sites (400). The paper is well written and the study is presented with sound logic. The nonlinear approach (Equation 3) is really interesting

and very puzzling from a mechanistic perspective. I personally am an advocate of this type of approach, thus I believe this paper, provided the authors address my comments below, would make a nice contribution to the audience of Biogeosciences.

My main (and only) concern about the paper begins precisely with the application of the nonlinear model and the suitability of the term 'ecohydrological' as a descriptor of the model. It is highlighted throughout the Abstract and Introduction that this model captures "coupled plant - soil moisture dynamics." Yet in my concept this assertion is misleading, because the model only captures plant dynamics (as shown in all figures) but not soil moisture dynamics per se. While the concept of feedbacks between V and W (Equation 3) is a novel one and important to present to the scientific community, I believe it is equally important to demonstrate that the model does indeed capture a realistic V/W feedback, or the hydrological part of the story. Page 8675 (Line 8) states: "its [this model's] parameters are more meaningful because the model aims to capture the fundamental feedbacks between soil and plant growth through a more process-based approach." While this statement is likely true (and I believe the processes described are true) this study does not specifically test the validity of this feedback. I express this serious concern because there is no assessment as to whether equifinality in W is an artifact in their model (my guess is that equifinality in W might be an artifact, given the spatial coverage that the model is applied to). I understand the authors acknowledge that they had no soil water content data, but could not the model be applied to additional sites where there is soil moisture data and prove its efficacy to capture these dynamics? I do not intend to sound obstinate on this topic, but I consider that demonstrating the ability of the model to capture soil moisture dynamics is simply fundamental to support their assertions.

That said, I suggest the authors modify the paper so that it first states that soil moisture varies across different spatial and temporal scales not only as a function of vegetation cover, but also as a function of variables like topography and soil texture (e.g., see Teuling and Troch, 2005). Second, I think it is important to show, even if it is at a handful of additional sites, that the model does capture the variability and responses of soil moisture to precipitation, before the reader learns about the results for all 400 sites. I suggest a 1:1 graph showing measured vs. observed soil moisture (again, even if it is at a handful of additional sites during a partial time of the year – this would be very informative). It would be a way to independently assess model performance before the model is applied to the entire study area. Otherwise, the 'hydrological' component of the 'ecohydrological' model becomes less powerful, and the proposed improvement of moving from Equation 2 to Equation 3 becomes less appealing; Equation 3 would have no more grounding than Eq. 2, yet it would have another parameter (which typically becomes the source of equifinality in models of this kind). The recommended assessment would provide confidence to sentences such as "more than two thirds of the NDVI-vegetation cover variability was explained by the linear and nonlinear models indicating a high responsiveness of these grasslands to soil water balance." (P 8675, L 18).

Concomitantly, the Discussion would require a series of statements indicating at which sites (e.g., dry vs. wet as shown in Figure 4) the model would be expected to work well and where it would probably not do so (this could be easily derived from the 1:1 assessment). This assessment would provide a level of confidence as to where future studies are likely to attain the best (and more realistic) performance of the model when capturing coupled plant - soil moisture dynamics, and it would certainly highlight the contribution of this manuscript.

Some specific Comments:

P 8662 Line 12. Briefly elaborate on what the implications/fundamentals of linear and nonlinear modeling are.

P 8663 Line 24. The second half of this paragraph is a bit dense and one has to read it two or three times before it makes sense. Please reword and simplify the statement: "Most of these" studies used linear modeling. . ." and onward.

P 8664 Line 14. Same comment as above, please reword: "Third, these grasslands are. . ."

P 8667 Line 27. It appears as if "In the linear model M1. . ." required a group of citations – whose model is M1?

P 8672 Line 21, Fig 2a. Please report r2 and p values of this positive relationship.

P 8675, Nice first paragraph of the Discussion.

References:

Teuling and Troch, 2005. Improved Understanding of Soil Moisture Variability and Dynamics. Geophysical Research Letters 32 (paper L05404) doi:10.1029/2004GL021935

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