Hydrol. Earth Syst. Sci. Discuss., 5, S359–S361, 2008
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Hydrology and Earth System Sciences Discussions

Interactive comment on "Ecohydrologic controls on vegetation density and evapotranspiration partitioning across the climatic gradients of the central United States" *by* J. P. Kochendorfer and J. A. Ramírez

Anonymous Referee #2

Received and published: 9 May 2008

The paper is well written, interesting and appropriate to HESS. I have major concerns on methods and results. Finally the paper is too long and should be reduced, for instance using appendix sections.

Comments:

1) It is not clear how peak LAI is estimated. It is clear that it is model estimated, and that a prescribed phenology during the year is used, scaling the monthly LAI with the peak LAI (pag. 660). I can't understand why the authors developed such approach

when a lot of vegetation dynamic models (VDM) are available and commonly used. And a lot of VDMs are coupled with hydrologic models now, and work very well. See Arora (2002) for a review. The authors may use a simple light-use efficiency approach, which is often used at monthly scale (e.g., Vertessy et al., 1996). In this way a dynamic LAI can easily estimated for each month. This is very important since the authors are using a constant-prescribed variation of LAI during the year, which may be a mistake. Indeed, the interannual variability of meteorological conditions can significantly affect monthly dynamics of LAI, and the prescribed phenology can alter the results. Hence, the authors should compare their approach with the use of a VDM, or at least justify its use in contrast with a VDM. They can easily couple SDEM with a VDM. Observed data of LAI are available, the coupled model can be easily calibrated.

2) I'm confused by the model calibration. The authors calibrated soil surface resistances (rss) and minimum stomatal resistances (rsmin) comparing modeled and observed runoff. But these two parameters are evapotraspiration (ET) parameters and only indirectly affect runoff. I think that runoff is much more affected by soil parameters, which directly affect the infiltration model, the soil water retention and the soil hydrodynamic. Why are you not calibrating the soil parameters? In my experience the sensitivity of runoff to these two land cover parameters is very low. You should make a sensitivity analysis first. Using a global multivariate approach for instance (see Franks et al., 1997). Moreover the authors show that the evaporation is not well simulated comparing observed and modelled data of deciduous forest. And this is not a good result for a paper that should improve the methods for ET estimates and partitioning. Hence, why are you not calibrating ET parameters comparing ET observations? I think that the model should be recalibrated, highlighting available observations and distinguishing the calibration of soil and vegetation parameters.

3) Pag. 658, rows 4-11: the methodology used by Kochendorer (2005) is not clearly described. This reference is not an international journal, and it is not indicated the type of publication. Probably an appendix can explain this methodology. However, I

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can't understand the use of a multivariate linear regression. Indeed, the authors are using the famous Rawls et al. (1982) database, which provides Brooks and Corey soil parameters for each soil texture. And in Figure 3 the soil texture map is provided. Hence, it should be very easy to derive the soil parameter maps. Then, again, soil parameters should be further calibrated comparing observed and modelled runoff.

4) Pag. 665: It is not possible to see the observed data of soil moisture in Figure 9. Hence, it is not possible to compare observed and model soil moisture. However, if you have soil moisture observations, why are you not calibrating soil parameters with these data? Again, there is confusion on data observed, soil and vegetation parameters, model calibration. Furthermore, when observed data are available, a calibration phase and a verification phase should be distinguished for testing the model. Such basic approach is not in the paper.

References Arora, V. (2002), Modeling vegetation as a dynamic component in soil-vegetation-atmosphere transfer schemes and hydrological models, Rev. Geophys., 40 (2), 3.1-3.26. Franks, S. W., K. J. Beven, P. F. Quinn, and I. R. Wright (1997), On the sensitivity of soil vegetation atmosphere transfer (SVAT) schemes: Equifinality and the problem of robust calibration, Agric. For. Meteorol., 86, 63 ;75. Vertessy, R.A., T.J. Hatton, R.G. Benyon, and W.R. Dawes (1996), Long-term growth and water balance predictions for a mountain ash (Eucalyptus regnans) forest catchment subject to clear-felling and regeneration, Tree Physiol., 16 (1-2), 221-232.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 5, 649, 2008.