Hydrol. Earth Syst. Sci. Discuss., 5, S338–S344, 2008
www.hydrol-earth-syst-sci-discuss.net/5/S338/2008/

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 #1

Received and published: 6 May 2008

General comments

The paper presents the application of an eco-hydrologic model to estimate the evapotranspiration (ET) partitioning for a large area of the central plains of the United States. The subject is appropriate for HESS and I recommend its publication.

The paper applies the Statistical-Dynamical Ecohydrology Model (SDEM) over thirty years in a large region with significant climatic and vegetation gradients and it

discusses the assumption that the maximum seasonal LAI is reached when the soil moisture reaches the point at which water stress is experienced. This approach has been already presented and discussed for some grassland sites by the same authors (Kochendorfer and Ramirez HESS, 2008), but not for a large heterogeneous region. While substantial conclusions on the validity of the maximum LAI approach are not reached, the data uncertainties and the model assumptions needed to estimate the ET distribution in a large region are clearly discussed.

Results show an overall realism in reproducing the different terms of the water balance, as well soil moisture. The paper gives an useful contribution for a better understanding of the role of plant water use in the soil-water balance. Special focus is given on the estimation of the evapotranspiration partitioning. While the approach used for the partitioning is quite standard, I agree with the authors that there is still a wide disagreement as to the relative magnitude of each component.

The background information throughout the paper are comprehensive, and I acknowledge the authors for their massive work of data collection and revision. However, this makes the paper very long, and not so easy to follow. This weakens the focus of the paper. Moreover, there is some degree of repetition of the contents of the Kochendorfer and Ramirez (2008) paper.

For this reason, I suggest the possibility to split the paper in two, one on the validation of the model results compared with the available data, another more focused on the discussion of the eco-hydrological implications. In particular, the interesting result that for water-limited vegetation relatively little variation in ET partitioning has been found can be better supported, as well the explanation that the higher (lower) soil moisture content in wetter (drier) climates is more-or-less completely offset by the greater (lesser) amount of energy available at the soil surface.

HESSD

5, S338–S344, 2008

Interactive Comment

As alternative, I suggest to shorten the paper, in particular the part on the application of the coupled models to the study region (paragraph 3).

Specific comments

1 Introduction

P 652, L 15-25.

The discussion of the Kochendorfer and Ramirez (2008) results, while important to motivate this paper, gives the impression that the paper will be focused on this issue, while it is also focused on the data and models results uncertainties. The goal of the paper should be specified with more detail here.

P 653, L 10. *In each year, the peak in green LAI was adjusted, up to a maximum of six, such that the critical soil matric potential is just reached in the latter part of the growing season.*

This key point of the methodology should be more clearly explained. What does exactly mean *to adjust*? It sounds here a little bit empirical.

P 653, L 10. It is not very clear the difference between the time series approach and the equilibrium approach. I suggest to mention and explain only the method used here.

3 Application of the coupled models to the study region

HESSD

5, S338–S344, 2008

P 657 paragraph 3.1 Soil hydraulic parameters.

I suggest to shorten this part or to move as Appendix.

P 659, L 5-15 paragraph 3.2 Storm statistics.

I suggest to skip this part, since it is already explained in Kochendorfer (2005).

P 660, L 2 *Surface albedo was taken from a gridded, monthly climatology created by Hobbins et al. (2001) based on Gutman (1988).*

Please discuss the relevance of this approximation. I8217;m wondering if there is a significant inter-annual variability of the surface albedo, and this can affect the energy balance components, and therefore the ET partitioning.

P 660, L 15 *keeping the phenology (seasonal progression) of LAI fixed.*

Does the phenology show a significant inter-annual variability? Please discuss.

P 661, L 22-3.

This discussion on the representativeness of the Buermann et al. (2002) dataset, while of some practical importance, distracts the reader from the main logical line of the paper.

P 662, L 15 *rsmin and rss.*

Those two parameters are very relevant for this paper, since their rate (weighted by the canopy fraction) basically controls the partitioning of canopy transpiration and evapo-

HESSD

5, S338–S344, 2008

ration, which is one of the main object of the paper.

Has been considered the effect of the wind speed attenuation in the canopy layer on Rss?

How is it possible to calibrate both Rss and Rsmin if the runoff is controlled by the total evapotranspiration?

A sensitivity analysis on the ET partitioning change for different choices of the Rsmin / Rss rate might be useful.

4 Results and discussion

P 664, L 15-20. Paragraph 4.1 Annual runoff.

In general, the lack of river network routing and lateral water distribution, along with other reasons indicated by the authors, can be significant limitation in estimating runoff. However, I agree that for this kind of model the observed agreement is sufficient.

P 665. Paragraph 4.2 Soil moisture.

Comparison with point observations. Since each grid cell is representative of a very large area, and point observation are representative of only local conditions, the comparison with point observations is affected by scaling issues and at the end it has little significance. It would be more appropriate to perform a single column simulation for each local observation location, with the same meteorological forcing of the model grid cell where the observation is located, but with soil, land cover and vegetation properties of the specific location where is the observation. In this way it is possible a more clean comparison of model results and observation.

HESSD

5, S338–S344, 2008

Paragraph 4.3. Model-determined leaf area index and above-ground net primary productivity.

In this part some relevant points are discussed: - The use of the critical matric potential to estimate peak green LAI. - The different behavior for humid versus more drier grasslands. - For which regions the model overestimates / underestimates LAI and why. - The presence of the inverse texture effect. - The impact of disturbances, grazing, crop production, irrigation. However, this discussion becomes dispersive, with a lot of details on the different datasets used. At the end it is not possible to have clear view of the model behavior and the paragraph becomes inconclusive.

I suggest to rearrange this part in a more compact way, focusing on the points outlined before. The needed references and the observations on the quality of the different datasets can be moved in Appendix.

The benefits and the limitations of the LAI optimization hypothesis should be more clearly and synthetically evidenced, as already done in the Conclusions.

Paragraph 4.4 Potential and actual soil evaporation.

P 672 Line 5-15. The discussion about the inverse texture effect is quite involved and needs rewriting.

Paragraph 4.5 Transpiration, total net primary productivity and water use efficiency for the grasslands.

I suggest to skip this paragraph, since the large uncertainties of the available observations do not let to draw clear conclusions. It seems a little too tentative section.

Technical corrections

HESSD

5, S338–S344, 2008

Fig 9. Numbers are too small.

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

HESSD

5, S338–S344, 2008