Hydrol. Earth Syst. Sci. Discuss., 10, C445–C448, 2013
www.hydrol-earth-syst-sci-discuss.net/10/C445/2013/

Hydrology and Earth System Sciences Discussions

Interactive comment on "Climate-vegetation-soil interactions and long-term hydrologic partitioning: signatures of catchment co-evolution" *by* P. A. Troch et al.

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Received and published: 22 March 2013

This is a very interesting paper that demonstrates that there are strong indications for co-evolution between climate, landscape and hydrology. By exchanging the climate and the parameterization of 12 catchments, each catchment is exposed to 12 climates and each climate acts on 12 catchments. Although all individual catchments fit the Budyko curve nicely with their own climate, when they are matched with alien climates they don't show the same emergent behavior. This is a strong indication that catchment properties (the parameterization in this case) have co-evolved with the climate to demonstrate consistent behavior, in line with the Budyko curve.

Although the authors speculate about the underlying reasons, the paper does not go much further than identifying the phenomenon. Clearly the ecosystem is key for understanding the linkage. The ecosystem of a certain catchment is the result of coevolution with the climate and the underlying substrate, resulting in an optimal root depth, interception threshold, soil structure, subsurface drainage structure, etc., so as to optimize the survival of the system under the given climate. In turn the ecosystem can provide feedback on the climate, which again is a higher level of co-evolution. I am sure that the authors also have ideas about this, but they chose to focus on the detection of the effect and to call for further research. I agree with that. It is important enough to show the hydrological community that this co-evolution is real and measureable and that we should investigate it further. The paper is an excellent example of how a Darwinian approach helps to enhance our understanding of the process of landscape formation and the link to hydrological behavior. By furthering our understanding of the process of co-evolution we can enhance our capacity to model and predict hydrological behavior.

I have two larger comments and a few minor comments:

1. The authors use the hsB model as the tool for analyzing the 12 catchments. Of course the authors could have used any other model that has sufficient physical basis to allow this analysis to be done. I have no problem with that. What I find hard to accept is the conclusion mentioned in the paper that the key variable that explains the tendency to produce more E/P is the "time scale that controls perched aquifer storage release". This observation follows from the fact that the hsB contains a perched aquifer. Other models that have a different mechanism for subsurface storage would have named this storage differently, e.g.: shallow groundwater, soil moisture, moisture stored in deeper layers available to the plant, or ... The essence is that there is subsurface storage of moisture available which vegetation can tap into by deep rooting, so as to extend the period of transpiration and to offset seasonality. I think the term used should be more generic and refer to the "time scale of subsurface storage release", or a similar term and to make it independent from the hsB model.

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2. The fact that deeper rooting or a larger interception capacity and hence more available storage for evaporation leads to higher E/P partitioning was also shown theoretically by Gerrits et al. (2009, WRR). The authors may not be aware of this publication. Related to Figure 2, she showed that more seasonality (in her approach a smaller number of rain-months per year) leads to lower E/P, while the availability of more subsurface storage (in her approach "carry-over" of moisture from one month to another) leads to higher values. The authors of this paper discuss the suggestion by Milly and Dunne (1994) that more seasonality can be offset by deeper rooting stating: "When a carryover factor, A, is used for semiarid areas (which takes into account that plants can withdraw water from deeper layers by developing a deep root system) the results improve significantly."

Minor comments:

1. Please don't use the term evapotranspiration (occurs 5 times in the text). Throughout the paper, the authors use the term evaporation (which I support), and I hope they agree to do that consistently.

2. I suggest that you use E_p (E underscore p) for potential evaporation and not the double symbol EP. The use of double symbol variables should be avoided.

3. Although Americans will know what MO means, this is not general knowledge, and moreover an unnecessary addition to the catchment name. I suggest in the text to remove ",MO" after Spring River. The first time the catchment is introduced, you can write Sping River, Montana.

Finally, I want to congratulate the authors on a very insightful and important paper.

References:

Milly, P. C. D., and K. A. Dunne (1994), Sensitivity of the global water cycle to the water-holding capacity of land, J. Clim., 7, 506– 526.

Gerrits, A.M.J., H.H.G. Savenije, E.J.M. Veling, and L.Pfister, 2009. Analytical deriva-

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tion of the Budyko curve based on rainfall characteristics and a simple evaporation model, Water Resources Research, 45, No. 4, W04403, p.1-15.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 2927, 2013.