Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2018-638-RC1, 2019

Hydrology and Earth System Sciences
Discussions

Interactive comment on "A global Budyko model to partition evaporation into interception and transpiration" *by* Ameneh Mianabadi et al.

Stephen Good (Referee)

stephen.good@oregonstate.edu

Received and published: 24 April 2019

GENERAL ASSESSMENT:

As shown by the recent multi-model assessment by Miralles (HESS 2016) or by Kumar (Remote Sensing 2018), contemporary estimates of the evapotranspiration subcomponents from models are wildly different. In many of these inter-comparisons, total evapotranspiration flux is relatively consistent between models while the divergence in the sub-components is large. This submitted paper presents another model, based on the Gerrits (WRR 2009) approach, that provides partitioned estimates of surface to atmosphere water flux. Results from this new model are compared with a few other models (GLEAM & STEAM), with differences between models broken down by land cover

type. This modeling and analysis are conducted in a satisfactory manner. However, it is hard to see how yet one more model that estimates evapotranspiration subcomponents moves us closer to a better understanding of these fluxes.

The introduction and a paragraph in the discussion relate this model to the Budyko framework. One possible way forward for the authors is evaluating how trends in flux components relate the energy and water limitations outlined by the Budyko framework, since this is the stated motivation of this model. This could move the paper beyond how it is currently presented as another land surface model applied using remote sensing observations. For example, see Figure 11 of Miralles's 2016 HESS paper for casting total evaporative fluxes in this context. Also relevant is the study of Good (Nature Ecology & Evolution 2018) which used a Budyko approach to examine how to partition evaporative fluxes. In revising the paper, I suggest the authors work to find how this approach helps us understand the different surface to atmosphere water flux pathways better.

Most critically, I find the language in this paper to be grandiose and predicated on a poorly based argument. As is written in the abstract and introduction, the authors suggest that others have "tried to improve the Budyko framework by including more physics and catchment characteristics . . . However this often resulted in additional parameters, which are unknown or difficult to determine." This statement, and others like it in this paper, is inappropriate for two reasons: (1) other approaches have used fairly easy to measure characteristics and (2) because the authors proceed to do exactly what they claim shouldn't be done by fitting "difficult" to determine parameters to optimize their results. For point (1) for instance, the approach of Porporato is explicitly physically based as is it dependent on the ratio soil water storage to mean rainfall depth which is a measurable quantity. Furthermore, both of these quantities are used in the analysis presented here. For reason (2), the 'b' parameter of this analysis, among others, is clearly stated by the authors (P5L15) to have been calibrated to produce the best results. This is very similar to the Li (WRR 2013) paper wherein the Budyko curve parameters were fit to vegetation cover. The authors use of language such as "tried" (P2L18) seems to imply these other authors were unsuccessful, which may not be true. In my opinion, this submitted paper is quite similar to these other efforts in that it has extended the Budyko framework with new parameters they have fitted based on physical processes. Here, the most important parameters dictating the transpiration component are when transpiration becomes downregulated, and how much maximal transpiration can be. Equation 17 needs more elaboration and justification, as does the parameterization of Sb as 50% of S_u,max. How were these values selected and what is the consequence of other using other values here. How much do these choices, and other values such as the 'b' parameter, influence model outcomes.

SPECIFIC COMMENTS:

P1L11: The 1/(1+f(phi)) is not the base of all Budyko curves. Budyko, himself used a hyperbolic tangent as an example. What do the lower and upper case f's represent? P2L33: This paper estimates available soil water capacity, not the actual soil water itself. Also, I wouldn't call these 'data' but modeled estimates. P3L16: Evaporation from 'non-superfical' soil moisture P4L11: Do you mean daily, not yearly, average? P5L14: I think you should also place these eqn in table 2 for consistency: A = b*S_u,max as well as Sb = 0.5*S_u,max P5L36: Reword here. As is stated above and in eq17, you do not hold D_t,m constant? Which is it? P5L38: Do you have a justification or citation for this statement? P7L17: No observations where used here. Only comparisons of the Gerrits model against other models. P8L42: There are many bare soil estimates (See the review by Kool 2014 Agg and Forest Met, for example). F2: Because of the size of these figures, and the large range of values, it becomes hard to discern differences. Why not plot the absolute value of E flux in panel A, and then the differences in panels B, C, and D. Consider this approach in later figures as well F3: Units for the RMSE, here and onward.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2018-638, 2019.