Dear Stephen Good,

We really appreciate your detailed review on our manuscript. The reactions to the comments are as follows.

General Assessment

Comment 1:

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.

Reaction:

Our aim is not to provide yet another LSM that partitions evaporation. Our aim is to show with a simple analytical model that the Budyko framework can be explained. For this we use the reasoning of the Gerrits model that recognizes the characteristic time scales of the different evaporation processes (i.e. interception daily and transpiration monthly). We revised the Gerrits-model in such way that it was possible to apply it at the global scale. As suggested by the reviewer, we will clarify this better and will relate the results of the model to the Budyko framework for a better understanding the partitioning of evaporation into transpiration and interception.

Comment 2:

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.

Reaction:

We thank the reviewer for this valuable suggestion. We agree that our aim was not clearly defined and also misleading in that sense. As suggested by the reviewer, we will evaluate the relation between evaporation fluxes and energy/water limitation in Budyko framework as provided by Miralles et al. (2016) and Good et al. (2017). In the graphs, we provide the Budyko framework for each land cover, and for the evaporation fluxes (E_i and E_t and E_{tot}), separately, to discuss how our model can be related to Budyko framework and how the energy and water limitation can be interpreted by our model.

Comment 3:

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.

Reaction:

Yes, you are right that we also have some calibration parameters and should therefore rephrase our text. Nonetheless, we think that we use a slightly different approach for these calibration parameters and other model parameters as well. Although others indeed also use 'measurable parameters', which could be tested in some case studies, some of these input values are not available at the global scale as for example the soil water storage. For example, carry over parameter (A) was available for 10 locations in Gerrits et al. (2009), but at global scale we did not have such data, so we proposed $A=b*S_{u,max}$, and we need to calibrate the "b" parameter to link A to a measurable variable. So, yes, we admit that you are right and we need to rephrase this part of the manuscript.

About the S_b as 50% of $S_{u,max}$, we mentioned in the text that in this study we assumed S_b to be 50% of $S_{u,max}$, as this value is commonly used for many crops, referred to (Allen et al. 1998). We will provide a sensitivity analysis in the revised manuscript to see how our results would change in response to the changes in the parameters.

Specific Comments:

Comment 4:

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?

Reaction:

As mentioned by Arora (2002), evaporation ratio (*E/P*) is a function of the aridity index (Φ) and Bowen ratio (γ) ($\frac{E}{P} = \frac{\emptyset}{1+\gamma}$). Arora interpreted the equation as follows:

"As a region becomes dry and is characterized by high potential evaporation, low precipitation and evapotranspiration, and high sensible heat fluxes then Φ→1, γ→1 and *E/P* tends towards unity implying little runoff. the other hand, as a region becomes wet and is characterized by low values of aridity index (Φ) and Bowen ratio (γ) then *E/P* < 1 and runoff occurs. Since Bowen ratio (γ) is also a function of available energy and precipitation (and thus a function of Φ) evaporation ratio may be expressed as a function of aridity index alone."

It leads to equation 1 in our paper. Thus, in equation 1, f and F are both mathematical functions, showing that *E/P* is a function of the aridity (Φ). F(Φ) can have many forms (exponential, hyperbolic tangent, etc.) as summarized in Table 1.

Comment 5:

P2L33: This paper estimates available soil water capacity, not the actual soil water itself. Also, I wouldn't call these 'data' but modeled estimates.

Reaction:

Gao et al. (2014) presented a new method where the available soil water is derived from time series of rainfall and potential evaporation, plus a long-term runoff coefficient. We agree that knowing soil moisture storage change is important for Budyko framework, but we use a method whereby we work around it by using plant available water. The method of Gao et al. (2014) provides plant available water (which is often linked to soil water capacity). In our paper we used it as $S_{u,max}$. We will rephrase it in the manuscript to explain it more preciously. Moreover, we agree that it's better to call this 'modeled estimates' rather than 'data'.

Comment 6:

P3L16: Evaporation from 'non-superficial' soil moisture

Reaction:

Thanks. We will add this.

Comment 7:

P4L11: Do you mean daily, not yearly, average.

Reaction:

Yes, daily average during the year. We will correct it in the text.

Comment 8:

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

Reaction:

Ok, will be done.

Comment 9:

P5L36: Reword here. As is stated above and in eq17, you do not hold Dt,m constant? Which is it?

Reaction:

We keep $D_{t,m}$ constant during the year (like $D_{i,d}$), but equation 17 shows that we calculated it as a function of the average yearly LAI. For water-constrained areas this is not a problem, because there $E_{t,m}$ is determined by the LHS of the min-function ($A + B(P_m - E_{i,m})$) as can be seen in Equation 7. For energy-constrained areas our assumption can be problematic. However, in those areas often temperature and radiation follow a sinusoidal pattern without complex double seasonality as e.g., occurs in the ITCZ. This implies that the overestimation of $E_{t,m}$ in winter will be compensated (on the annual time scale) by the underestimation in summer time.

Comment 10:

P5L38: Do you have a justification or citation for this statement?

Reaction:

Please see our response to comment 9.

Comment 11:

P7L17: No observations where used here. Only comparisons of the Gerrits model against other models.

Reaction:

It is a general sentence for Taylor diagram, not only for our model. But to make it clear, we change "This diagram can provide a concise..." into 'A Taylor diagram can provide a concise..."

Comment 12:

P8L42: There are many bare soil estimates (See the review by Kool 2014 Agg and Forest Met, for example).

Reaction:

We meant that there is hardly any data on the forest floor interception storage capacity (S_{max}). We did not intend to refer to bare soil evaporation.

Comment 13:

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

Reaction:

Thanks for your suggestion. We did it before, but the single pixel outliers may blow up the entire figure what was also not a good way of showing the differences. That's why we moved towards the Taylor diagrams. Moreover, we also wanted to show the original data.

Comment 14:

F3: Units for the RMSE, here and onward.

Reaction:

We used normalized RMSE in these figures as shown in the following equation, so it has no unit.

$$\mathrm{NRMSE} = \frac{\mathrm{RMSE}}{\overline{X_O}} \qquad (1)$$

In this equation, NRMSE is normalized RMSE and $\overline{X_O}$ is the average of observation values (here the values estimated by Gerrits' model).

References

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