Review of HESS manuscript #hess-2017-306

Title: A simple global Budyko model to partition evaporation into interception and transpiration
Author: Mianabadi et al

This manuscript describes a global model of land surface evaporation that uses remotely sensed data to estimate various parameters of the original "Gerrits 2009 WRR" model.

The topic is important and within the scope of HESS.

One of the main reasons to use the "Gerrits 2009 WRR" model is that it was based on underlying reasoning that explicitly recognises the characteristic time scales for different processes, e.g. evaporation of water intercepted by the canopy has a characteristic time scale of order one day (see p. 3, lines 17-29 for relevant background).

However, as currently written I could not see how the manuscript followed this approach.

For example, as currently described (and I am not convinced the description is accurate), the daily potential evaporation is calculated as the annual potential evaporation divided by 365 (see p. 4, line 7). This means that every day of the year has the same potential evaporation. This means that there is no seasonal variation in the atmospheric demand imposed on the evaporation of water intercepted by the canopy.

The situation for transpiration is more or less the same. For example, the monthly transpiration threshold is calculated as the annual potential evaporation divided by 12 (see p. 5, lines 20-31). This means that every month of the year has the same transpiration threshold that is subsequently modified using the Novak and Jan (2005) formulation along with remotely sensed LAI.

While the focus of the paper is on annual totals, we know in terms of the underlying processes that seasonal variations are important. One justification of using the "Gerrits 2009 WRR" model is to explicitly recognise the time scales of the underlying processes. A seasonal time scale is also important and I do not see how potential evaporation being the same for every day/month of the year recognises the seasonal time scale? With that in mind, the two comments noted above seem major oversights.

Now I am not convinced that this is actually what has been calculated, but it is what has been described in the manuscript. If this is what has been calculated then the problem could be easily detected by examining some seasonal cycles in different regions. In fact an evaluation of the mean seasonal cycle of the new approach compared with other approaches could be considered useful in evaluating the new approach. However, all results (Figures 2-9, Tables 3-6) are presented as annual totals so the seasonal variations are not evident. Perhaps these seasonal cycles have been done?

Finally, in the formulation of the methodology you describe the basic equation, Eqn 2 (p. 3, line 6). After some discussion, there is an amended description for water bodies and for all other surface types (Eqn 3). However, the term Es from Eqn 2 has gone missing. The implication is that the model does not consider evaporation from soil. I assume the model actually does calculate soil evaporation - it is just the overall description that does not consider this term.

I do not see that this manuscript was anywhere near ready for submission.

Recommend: Reject.

Other Comments:

1. Table 2 has been a useful start. Rather than present this as a two column table of equations, why not have one column of equations, another for units and a final column for explanation of each term. Every variable in the paper needs to be here. As it currently stands some terms are explained in the table and some in the text and it is hard for a reader to follow all the different terms.

2. Fig. 1c. The scale bar for LAI has a maximum at 60. This seems a little excessive? Is there a numerical problem in the figure?

Michael L. Roderick, 23-06-2017