The authors would like to express their sincere gratitude to the Anonymous Referee (Reviewer 2) for his/her useful comments. The reactions to the comments are as follows.

Comment 1:

Equation (3b): E = Ei + Et. Since Ei includes soil evaporation, I would suggest to interpret this as ET = E +T where E is evaporation and T is transpiration.

Reaction:

As suggested by Savenije (2004) and based on the definition of total evaporation provided by Shuttleworth (1993), we call the sum of interception (E_i), soil evaporation (E_s), transpiration (E_t), and evaporation from water bodies (E_o) as "evaporation" (E). Thus, we did not apply the term of "evapotranspiration" (ET), because we agree that they "are very different in terms of time scale, time of occurrence, physical characteristics, climatic feedback and isotope composition" as explained in Savenije (2004).

Comment 2:

Page 3 Line 21: Does Et have the same definition as the Es defined in Line 16?

Reaction:

Yes, as mentioned in reaction to comment 1, E_t is transpiration which is evaporation from the soil moisture connected to the root zone, because that's where the trees get their water from.

Comment 3:

Page 4 Lines 28-30: whys accounting for n is rarely necessary? Maybe it is better to explain it briefly here.

Reaction:

Miralles et al. (2010) and Pearce and Rowe (1981) both mentioned that accounting for n is rarely necessary. Pearce and Rowe (1981) mentioned that "In many climates, however, such adjustments will not be necessary, or small enough that they can be neglected". In our interpretation this is because the number or times the interception storage can be filled and completely emptied is limited once we assume a drying time of ca. 4 hours, which is common.

For 12 hours of day light, it means that n can be maximal 3 times. However, the chance that you have 4 storms every 4 hours, with a drying period of 4 hours, is rather small for most climates.

Comment 4:

Page 5 Line 36: If the inter-annual variability of the Dt,m has any impact on the results?

Reaction

We explained in the manuscript (page 5, line 36-38) that taking a constant value for $D_{t,m}$ can be problematic in energy-constrained areas. 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.

In addition, Gerrits et al. (2009) provided a sensitivity analysis on the effect of different $D_{t,m}$ on total evaporation. Their results showed that total evaporation is sensitive to $D_{t,m}$ only once the annual rainfall exceed ±1000 mm/y.

Comment 5:

Page 5 Lines 37-37: "But in those relatively wet areas transpiration is underestimated in summer, but overestimated in winter, which will cancel out on the annual scale." Delete the first "But"?

Reaction

Thanks, it will be done in the revised manuscript.

Comment 6:

Page 7 Line 32: year-1

Reaction

Thanks, it will be corrected in the revised manuscript.

Comment 7:

Page 8 Lines 2-3: Is there any analysis in this study to demonstrate that the precipitation is the major factor that caused the different results from different models?

Reaction

By providing a sensitivity analysis in the revised manuscript, we can show how the model is sensitive to precipitation. Moreover, the results of sensitivity analysis conducted by Gerrits et al. (2009) shows that the results are significantly sensitive to change in $n_{r,d}$. We revised the manuscript as follows:

"Different precipitation products applied in the models is likely the reason for the differences. As found by Gerrits et al. (2009), the sensitivity of the model to the number of rain days and rain months especially for the higher rates of precipitation can be a probable reason for poor performance of a model especially for evergreen forests with the highest amount of precipitation."

Comment 8:

Page 9 Lines 27-32: The global transpiration ratio estimated by Gerrits' model is larger than nearly all of the other studies listed, is there any reason?

Reaction

Our transpiration ratio estimate is indeed in the higher range compared to other models/studies, however, the transpiration ratio estimated by Miralles et al. (2011) is higher than our model (80% in comparison to 71%). Moreover, our estimation is close to that of Sutanto (2015) (69%) and Good et al. (2015) (65%). Coenders-Gerrits et al. (2014) also found that based on the model of Jasechko et al. (2013) transpiration ratio changes between 35% and 80%, which is in line with our current findings.

Comment 9:

Page 10 Lines 27-29: Since the constant value of 0.935 mm in Equation 10 could be underestimated for the forest floor interception, then what value is advised for the forest floor?

Reaction

Forest floor evaporation can be modeled for each region based on its characteristics (e.g., Wang-Erlandsson et al. (2014)). Or typical values on S_{max} for the forest floor can be found in Table 22.1 of Gerrits and Savenije (2011). For example, in the UK for Pine (Pinus sylvestris), it is 0.6-1.7 mm (Walsh and Voigt, 1977), in Australia for Eucalyptus, it is 1.7 mm (Putuhena and Cordery, 1996) and in Luxembourg for Beech (Fagus sylvatica), it is 1-2.8 mm (Gerrits et al., 2010).

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