Comments to the Author:
Dear Authors,

the reviews of your manuscript were extremely positive, I have decided that minor revisions are necessary before the manuscript can be published. I agree with the reviewers that this manuscript is very readable and will likely be well received by the community. In revising your manuscript to address the various issues highlighted by the reviewers can I ask you to consider a few points from me as well.

Thank you

1. The algorithm descriptions covered in the appendix are extremely useful for the community. I note that it is referred to once in the introduction but it would be great if you could find at least one more place in the manuscript to refer to the appendix (perhaps the discussion?). I think what you've done here is very valuable and it would be a shame for a reader to miss this.

Thank you for this suggestion; we did put quite a bit of work into the Appendix but felt that it added too much length to the text. We refer to the Appendix in the Introduction, and in section 3.1 and 3.3. We added another reference in section 3 and in the Conclusions section.

2. In 2.1 where you talk about the CMIP models having a T/ET ratio of 0.22-0.58 I think it would be valuable to offer some insight into why: (a) they disagree with each other; and (b) why this ratio is noticeably below other data-based estimates. This is an optional suggestion but I do think given this is a review it would be good to inform the reader. Perhaps they might consider citing Berg and Sheffield - Evapotranspiration Partitioning in CMIP5 Models: Uncertainties and Future Projections. Similarly, you might wish to more explicitly raise the issue of discrepancies amongst how models simulate LAI and the impact this has on the water cycle / ET partitioning.

We would also like to know more about the reasons for the discrepancy but Wei et al. (2017) only note that the reason is due to methodological differences without discussing in detail why, perhaps due to the short format of Geophysical Research Letters. Additional explanations are also not available in the supplement of Wei et al. (2017). This strikes us as an important avenue of future research.

3. Again feel free to ignore this, but this is one of the few papers I've seen raise this issue. The authors neatly raise the issue of interception. In our 2013 GCB paper on WUE (Forest water use and water use efficiency at elevated CO2: a model-data intercomparison at two contrasting temperate forest FACE sites), we found that the proportion of intercepted water varied among the models by between 2-14%. This was considerably below the field estimates for the sites (and the range you quote in 3.6). It was striking how data free the assumptions were than underpinned how interception is treated in models. I'm not suggesting you get into how models simulate interception, I just think you might consider highlighting this is a serious problem for models and may contribute to erroneous partitioning ratios - see above.

Intercepted water is very difficult to measure and we were fortunate to have an expert (Dr. Shuguang Liu) help with a subsection on it. We added the findings of De Kauwe et al. 2013 to further emphasize its importance for models.

3. The paper didn't seem to make much of soil evaporation? I realise it is a minor component of total ET, but recently we noted how poorly this was simulated by models. In a water-limited, semi-arid ecosystem, some models thought soil evaporation was around 50-130 mm yr-1, whilst other models thought it was 2-3.5 times greater (Challenging terrestrial biosphere models with data from the long-term multifactor Prairie Heating and CO2 Enrichment experiment). I only raise this example because it suggests to me that this isn't a trivial process to model (otherwise there wouldn't be this disagreement). I was expecting to see some sub-section on soil evaporation, but this may simply be my personal bias on this issue, so ignore as you wish.

Following referee comments, we now added a section on soil evaporation following the new manuscript by Or and Lehman (2019). The reason for our very brief discussion of soil evaporation before is that other reviews have covered it. Now, with new analytical techniques for estimating it, we agree with the reviewer and added a subsection to the manuscript and cite De Kauwe et al. 2017.

4. In table 2 where the variability in the exponential term is shown across models, I feel whilst interesting - without some context or explanation, it is a bit limited in terms of insight. Could the authors group the models by their stomatal assumptions (Ball-Berry, Leuning, etc). Does this help explain why they vary? Why is the BEPS model most similar to the observations?

We are not entirely sure why BEPS is most similar to observations but find that it is interesting that it does. We did not want to pursue a long intercomparison of models versus measurements in the present manuscript, and this comment combined with the comment above made us realize that a stand-alone multi-model intercomparison of CMIP5 and other models with respect to evaporation and transpiration partitioning would be forthcoming.

5. Was there a reason the ECOSTRESS mission wasn't mentioned in section 5?

We now mention ECOSTRESS explicitly in section 5.

6. When the authors discuss partitioning via the use of GPP in the WUE approaches, it would be worth mentioning that GPP isn't strictly an observation (so any errors in GPP will propagate here).

Thank you for pointing this out, we added a passage about GPP uncertainty to section 3.

Best wishes,

Martin