

## ***Interactive comment on “Evapotranspiration in the Amazon: spatial patterns, seasonality and recent trends in observations, reanalysis and CMIP models” by Jessica C. A. Baker et al.***

### **Anonymous Referee #1**

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This article presents original estimates of evapotranspiration (ET) derived via water balance in the Amazon and several of its subbasins. The authors use these estimates to examine spatial variability in ET within the Amazon, seasonal and interannual variability in ET at the subbasin and Amazon-scale, and the dominant drivers of ET variability in the region. The authors additionally compare their findings to existing ET data from an extensive suite of remotely-sensed ET products, site-scale ET from flux towers, reanalysis models, and CMIP GCMs to evaluate how well they capture the dynamics of Amazonian ET.

I commend the authors for writing a clear and detailed article that will be of interest to

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both climate modeling and tropical ecohydrology audiences. Their references to prior literature are thorough and concisely summarized, their findings are presented in well-designed figures, and the details included in the supplementary information will allow readers to build a deep understanding of their analysis. I am also happy to see a clear explanation of error propagation in the paper, which is important for any study using a water balance approach as a benchmark for more complex models.

I do not think any major revisions or additional analyses are needed, but I have some comments and questions listed below that might improve the paper:

L24: The abstract reports a “strong seasonal cycle in basin-mean ET controlled by net incoming radiation...”. Here and elsewhere throughout the paper I would consider slightly softening the language, e.g. by saying that basin-wide ET is “primarily controlled by radiation” or “highly correlated with radiation” or something similar. My concern is that while the current statement is consistent with your findings, it is very general and may mislead unfamiliar readers into neglecting other important dynamics behind Amazonian ET. As your results and many other papers have found (Maeda et al. 2017 in ESD comes to mind), vegetation dynamics and water availability (via rainfall as well as terrestrial availability) appear to modulate the ET signal in certain regions during certain times of the year. You have demonstrated quite well that, when aggregated across the entire basin, ET and Rnet have strikingly similar seasonal cycles. But I would be cautious about unintentionally implying radiation is the sole control on ET’s seasonality when the full story is more complicated.

L55: The comma after ET seems unnecessary to me.

L199: There appears to be some formatting mistake with the uncertainty term here.

L227: If pasture is the dominant regional land use, why exclude these tower sites? Is pasture just not common enough across your subbasins to be reflected in your tower analysis?

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L234: Suggest removing the “to” in “near to Manaus”

L249: It’s worth including the version of MOD16A2 that you used, presumably version 6. I would say the same thing for P-LSH and CHIRPS if there are publicized version numbers associated with the data (I think CHIRPS is on version 2 these days).

L279: Why re-grid to 1x1 degree pixels for the visualizations? To reduce noise? I would mention somewhere what the native resolutions of the CMIP models are before resampling.

L295: How many of these gaps were there? If I’m understanding this correctly, each time series is only 11 datapoints, so filling any of those datapoints with climatological means could obscure trends quite a bit. Worth mentioning somewhere in the text or Supplementary Info how much gap-filling was necessary for each basin’s ET time series.

L304: On first read I was a bit confused by the term “corresponding sources.” In fact this whole paragraph could use some small tweaks for clarity. This reordering of the sentences (and complete removal of the “corresponding sources” sentence) sounds better to me, but you have already demonstrated that you are a capable writer so I will trust you to make whatever changes you see fit:

“...Bins with fewer than five data points were excluded from the analysis. Satellite-based ET estimates were binned according to precipitation from CHIRPS, radiation from CLARA-A1 (Karlsson et al., 2013) and LAI from the MODIS MOD15A2H product (Myneni et al., 2015), each re-gridded to 0.25° x 0.25°, while reanalysis and model ET were compared with reanalysis and model variables, respectively. For ERA5, we used the ‘high vegetation’ LAI field since the Amazon is predominantly covered with tropical forest, though repeating the analysis with ‘low vegetation’ LAI made little difference to the results. Note that the satellite-based MODIS ET...”

L306: I would appreciate some very brief discussion of how trustworthy CLARA and

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MOD15A2 can be considered in this region, if anything is known. For instance, does MOD15A2 suffer from the sun-sensor geometry issues known to affect MODIS in the Amazon? I don’t know much about the CLARA-A1 dataset but perhaps there is some validation study available, or at least you could explain why you chose it over other radiation datasets.

L310: Was the Amazon’s hydroclimate during 1994-2004 broadly similar to 2003-2013? I’m not immediately familiar with the Amazon’s recent climate trends, but if one period had worse droughts or wetter wet years than the other, mean ET may show a response to that. Somewhere in the paper, one or two lines covering this question might be a good addition.

Figure 3: Is there some kind of interpolation or smoothing being used here? My understanding was that the CMIP models were resampled to 1-degree grid cells for visualization, which seems much coarser than the data in panels g and h. I don’t necessarily take issue with interpolating for the presentation of this figure, but it may provide a false sense of spatial detail and should be explicitly stated somewhere.

L354: I don’t know if I’d say the tower gradient is “similar” given so few datapoints, and the two northeastern towers featuring such different values. Perhaps just say they appear to display an east-west gradient. Some explanation of why the two very close towers feature quite different mean ETs would be welcome as well—is it a land cover difference?

L377: As I wrote regarding the abstract, I would be careful with statements like “water availability is not a limiting factor controlling the spatial distribution of ET over the Amazon.” It seems to me that P may well limit ET in some catchments (e.g. Madeira, which has relatively low ET and P but relatively high RDN in your plots. Maeda et al. (2017) characterized Madeira and other basins as water-limited for at least part of the year). I think you could justifiably conclude “water availability does not consistently limit ET in all regions of the Amazon” from Figure 4, or that “water availability does not limit ET as

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consistently across Amazonian subbasins as radiation.” But as it is now, I think the line is potentially misleading.

Figure 4 (and others): The symbols for Amazon and Tapajos are difficult to distinguish without zooming in. Fortunately you have prepared your figures well enough that zooming in is possible, but I worry that people reading printed versions of the article would still have trouble. Maybe Tapajos could be replaced with an unfilled circle?

Figure 7: I am curious why you decided to do this analysis on monthly climatological means rather than just plotting LAI and ET in every individual month you had available. Also, it looks like you are calling P-LSH “LSE-Zhang” in the plot titles; I would change this to be consistent throughout.

Supplemental Information:

Table S2: Please clarify what area these values pertain to.

Table S6: Change Zhang to P-LSH?

Figure S4: In last sentence of the caption, change “was” to “were.”

Figure S7: I would love to see this figure replicated for RDN and LAI, since so much of your analysis of various ET data sources hinges on the comparisons to their respective RDN and LAI datasets. After reading your article I was left wondering how spatially variable these drivers are in the reanalysis and GCMs.

Figure S9: If it’s not too much trouble, a similar plot showing LAI’s seasonal cycle across the basin would be interesting since LAI appears to relate to ET when viewed across subbasins and months. Adding to this figure may make it too crowded, so perhaps just in a new supplemental figure.

Figure S10: I don’t understand what the colors in the caption are referring to. Where is dark blue? What about red and magenta?

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2020-523>, 2020.

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