We would like to thank Dr Matthew Rodell and one anonymous reviewer for their excellent suggestions and comments. We are confident that the manuscript has been greatly improved after addressing their suggestions. We have expanded and improved the description of our methods, to avoid any misinterpretation on how the components of the water balance were obtained. Furthermore, as suggested by Dr Rodell, we included uncertainty analysis for the ET estimates, which is now presented in several figures (including extra figures in the supplementary material) and discussed in the text.

Detailed replies for each comment are provided below:

Comment: Equation 2 is incorrect. It should be

$dSn = (TWSAn{+}1 - TWSAn{-}1)/((n+1)-(n-1))$, or equivalently,
$dSn = (TWSAn{+}1 - TWSAn{-}1)/2$,
as shown in equation 13 in Swenson and Wahr (2006). This error leads to dS, and consequently monthly/seasonal ET, being inaccurately estimated throughout the manuscript.

Reply: We believe a lack of details in the description of our methods might have misled the reviewer. We have improved this section to clarify the dS/dt calculations. Equation 2 refers only to the *dS* component, which is later divided by *dt*. Hence, the *dS/dT* component was indeed calculated similarly as in Swenson and Wahr (2006), in the sense that we have properly accounted for the inherent temporal sampling of GRACE. In our case, as the unit used in our water balance equation was mm month^{-1}, we have first divided *dS* by the number of days between GRACE observations, and then multiplied by the number of days in the month. Another detail is that we performed centered differences for calculating *dS* (instead of forward or backward differences, as suggested in Landerer et al 2010). For this, we have adjusted the TWSA values for the beginning ($TWSA_{n-1}$) and end ($TWSA_{n+1}$) of the respective months, resulting in a *dt* of 3 months, consistent with the three-month sliding window used for P and R. As pointed out by the reviewer, failing to correctly calculate the changes in water storage would lead to inaccurate ET estimates, which would probably be evident in our results.

Comment: There is no uncertainty analysis provided for the water budget ET estimates. It should be included, and could be accomplished by computing the square root of the sum of the squares of the P, R, and dS errors. See, for example, Rodell et al. (2011).

Reply: This was an excellent suggestion. We carried out the uncertainty analysis as suggested. The calculations were made as described in Rodell et al. (2011), but with a small difference in the assessments of rainfall errors. In our case, we assessed rainfall errors using TRMM 3B43 relative errors layer. Interestingly, we observe that rainfall is shown to be the main source of uncertainty. We have added the 95% confidence limits in the figures showing the ET seasonal pattern. The uncertainties for each component of the water balance are shown in new figures at the supplementary material.

Comment: Page 2, line 30 – Add to the literature review that Rodell et al. (2011) applied the water balance approach (with observed precipitation, runoff, and GRACE terrestrial water storage) to ET estimation over the Tocantins River basin (among others) and found that the seasonal cycle of ET in that basin is weak.

Reply: We have added this literature review. Nonetheless, we would like to point out the fact that the Tocantins River basin is not part of the Amazon River basin. The vegetation cover across the Tocantins River basin is also quite different, being mostly covered by Cerrado (Savannas).

Comment: Page 7, line 6 – "are" should be "is" (the subject is "analysis").

Reply: Corrected as suggested.

Reviewer #2

Comment:

For equation 2: if for any given month n, the monthly change in water storage is defined as

$dS_n = TWSA_n - TWSA_{n-1}$

then, the three-months sliding window should be

$dS_n = TWSA_{n+1} - TWSA_{n-2}$

assuming that the window includes both month that precedes and follows month n. This should be corrected and ET recalculated accordingly.

Reply:

This is probably a misunderstanding. We improved the description of the methods to clarify this point. Given that GRACE provides monthly mean TWS anomalies, the dS calculation has to be adjusted to account for this temporal sampling. There are different approaches for calculating dS, including forward and backward differences (Landerer et al 2010), as suggested by the reviewer. However, due to errors and uncertainties in GRACE data, these approaches may result in high-frequency artifacts (Landerer et al 2010). To overcome this problem, we carried out centered differences for dS calculation. That is, dS for a given month *n* was estimated considering the accumulated fluxes from the beginning of month *n-1* to the end of month *n+1*. Hence, we considered changes in TWSA from the first day of month n-1 to the last day of month n+1 (i.e. separated by 3 months). For this procedure, TWSA values had to be adjusted for the beginning and end of each month, as described in page 4.

Landerer, F. W., J. O. Dickey, and A. Güntner (2010), Terrestrial water budget of the Eurasian pan-Arctic from GRACE satellite measurements during 2003–2009, J. Geophys. Res., 115, D23115, doi:10.1029/2010JD014584.

Comment: Figure 3 would highly benefit of having consistent units between radiation, precipitation and evapotranspiration. It would also benefit of a superposition of radiation and ET. The addition of runoff would add important information. Even though runoff is a major part of the model equation, it is never shown in the figures, making it harder to understand the entire water balance. Also, the figure should probably show dS/dt and not TWSA, which is the term used in the equation and the next figure.

Reply: These are good suggestions. We changed the radiation unit to equivalent evaporation i.e. mm $month^{-1}$. Furthermore, we added new figures in the supplementary material, showing all the components of the water balance, including runoff and dS/dt, as you suggested. This information was not added in Figure 3, to avoid overloading the plots. For instance, superposing radiation and ET would hinder the visualization of ET patterns, which are the most important information in this figure. We, however, included a figure superposing radiation and ET in the supplementary material (Fig. S2).

Comment: Another interesting addition to figure 3 would be to add MODIS PET. It is used in figure 4 to calculate the Budyko dryness index, but understanding its relationship to ET in figure 3 would be very useful. A graph with radiation (unit converted), ET and PET together would be beneficial.

Reply: The suggested plots were added in the supplementary material.

Comment: Figure 4 has inconsistencies. There are problematic negative values of AET/P-dS in the Solimões graph that are not present in the all-basin graph. If those values are correct, there must be problems in the data that are not mentioned.

Reply: This is perhaps a misunderstanding. There aren't any negative values of AET/P-dS in the plots. This confusion was probably caused because a slight difference in the y axis origin. The origin was set to zero for the "all-basin" plots, while in the plots for the individual basins, the origin was set to 0.2. The plots were corrected, and the figure was improved based on your comments (below).

Figure 4

- A great proportion of the figure is blank. It would be much clearer if the scale on the y-axis was only up to 1, therefore spreading out the data points

- Why is the graph not positioned at (0,0)? Why are there negative values of AET?

- The legend doesn't need to be repeated 5 times, thus gaining some space for the data

Reply: The figure was modified as suggested. Y-axis limit was set to 1.2, and the origin was set at (0,0). The repeated legends were removed from the plot, and a single legend inserted under the figure.

Comment: Figure 4 is the main source indicating the water-limitation in Purus, Madeira and Tapajós. Even though, PET/P-dS > 1, most corresponding values of ET/P-dS are not close to 1, suggesting that there is a lot of water that is not used/available for ET, or that ET is underestimated. More discussion should address this point. The addition of another source of PET (derived from the Radiation data, with a Priesley-Taylor equation for example) could also add some information.

Reply. An additional Budyko plot, made using an alternative source of PET, calculated from a variant of the Penman–Monteith formula, was included in the supplementary material (Fig S3). This new PET dataset was obtained from the high-resolution grids of monthly climatic observations (CRU TS3.23) (Harris et al., 2014). The grid is constructed using monthly observations at meteorological stations. Due to the sparse network of ground meteorological stations over the amazon region, we will maintain the MODIS PET data as the primary dataset in the manuscript.

Harris, I., Jones, P. D., Osborn, T. J., & Lister, D. H. (2014). Updated high-resolution grids of monthly climatic observations - the CRU TS3.10 Dataset. International Journal of Climatology, 34(3), 623–642. https://doi.org/10.1002/joc.3711

Comment: In short, figure 5 does not add valuable physical understanding of ET in Solimões...

Reply: Figure 5 was removed from the manuscript.

Comment: Figure 6 aggregates all basins into one figure, which can be debatable if there are to be differences in ET responses to climatic drivers in different regions. Nonetheless, the lowest ET values are mainly located in the mid-range of both radiation and rainfall, which is not discussed. High ET in relative low radiation and high precipitation could indicate that the ET is not limited by either climatic factor. On the other hand, the relatively low ET values when both radiation and precipitation are low can indicate a combined effect or either one of them, but with the units not being consistent it is hard to interpret. In short, the figure is not discussed in its full potential, and claims are weak.

Page 7, lines 23-27. See comments for Figure 6 above.

Reply: These are good suggestions, we agree with the reviewer. We changed the radiation units in Figure 6 to same unit as rainfall to facilitate comparison. We also improved the description and discussion of these results, as suggested by the reviewer. We consider that the fact that the lowest ET values are located in the mid-range of both radiation and rainfall are likely due to the influence of other variables, in particular soil water storage and root access to deep water. This argument is reinforced by the fact that at this radiation range (~ 200 mm $month^{-1}$), when rainfall drops below 100 mm $month^{-1}$, an increase in ET cannot be explained by water input from rainfall. The discussion of the figure has been further developed to clarify this argument.

Comment: Page 6, line 25-26. The sentence “Furthermore, …” is unclear because if part of the basin is at higher elevation with less rainfall, then it does not explain why the annual rainfall is still similar to that in Purus, or the relatively low ET.

Reply: In fact, the mean annual rainfall rates cannot provide enough information to explain these differences in ET. As shown in our Figure 1, the spatial distribution of rainfall rates in these two basins varies significantly. While rainfall in Purus is uniformly distributed (~2000 mm $year^{-1}$), in the Solimões basin we see a mixture of very wet (>3500 mm $year^{-1}$) and dry (<500 mm $year^{-1}$). The sentence you are referring to, is simply pointing to this spatial heterogeneity as a possible cause for the differences in ET.

Comment: Page 7, lines 1-3. ET>rainfall does not indicate a limitation in water availability, because of possible water storage and root access to deep water. Even if the lowest ET values were found when rainfall was in decline, ET increases in the lowest rainfall months. Those statements need to be rephrased.

Reply: We agree with the reviewer. These statements were rephrased to account for the possible influence of soil water storage and root access to deep water in compensating the rainfall deficit.

Comment: Page 7, lines 8-10. There is confusion between seasonal and inter-annual variation in that first sentence. ET shows small seasonal variation (or none) but huge inter-annual variation (figure 3), but it does not read that way! As stated, that basin is probably operating at PET all year, having enough energy and water all the time (but that could be interesting to see, with added PET on figure 3).

Reply: This sentence does not contain any remark about inter-annual variation. The sentence mentioned by the reviewer refers to "intra-annual" variation, that is, variations occurring on a time scale of less than 1 year.

Comment: Paragraph 3.3 is very interesting. More emphasis could be put on the fact that the relationship between greening and ET is weak (low r2 and low/no slopes). The best correlation is in Solimões where the relation is opposite to the one in the other basins. In Tapajós, there is no correlation between EVI and ET, if anything ET is more variable (scattered) with higher EVI.

Reply: We agree that these are interesting results. We have further developed the manuscript to include improved discussion on this specific topic. In particular, we have emphasized the biophysical causes of EVI seasonality, and how they may be related to ET. We also examined previous studies evaluating the relationship between forest functioning and EVI, to discuss how our results may contribute to better understand climate-vegetation interactions in this region.

Comment: Page 8, lines 15-18. In the Solimões basin, the two models agree in their seasonal pattern of ET. They can be both misrepresenting what is happening, but given that the water-balance estimate of ET is hard to understand, some discussion about the water-balance method uncertainties (here or in paragraph 4.3) seems necessary.

Page 11, lines 10-15. Even though water balance methods are adequate alternative, the entire discussion lacks any perspective on uncertainties. In particular in places were ET seasonality is difficult to understand, like in Solimões.

Reply: We agree. We have extensively improved the assessment of uncertainties in the manuscript. We included the 95% confidence intervals of ET estimates in figures 3 and 9. The uncertainties were calculated by combining measurement errors on P, R, and dS/dt, as suggested in Rodell et al (2011). The approach has been fully described in the methods section. Errors in GRACE TWSA estimates were assessed using gridded fields of measurement and leakage errors provided with GRC Tellus data (Landerer and Swenson, 2012), while uncertainties in monthly rainfall values were assessed using the rainfall relative error layer available in the TRMM 3B43 product (Huffman, 1997).

Huffman, G. J.: Estimates of Root-Mean-Square Random Error for Finite Samples of Estimated Precipitation, J. Appl. Meteorol., 36, 1191–1201, doi:10.1175/1520-0450(1997)036<1191:EORMSR>2.0.CO;2, 1997.

Landerer, F. W. and Swenson, S. C.: Accuracy of scaled GRACE terrestrial water storage estimates, Water Resour. Res., 48(4), 1–11, doi:10.1029/2011WR011453, 2012.

Comment: Page 9, lines 26-30. This discussion would suggest that there is no water-limitation. This is inconsistent with figure 4 and some previous comments and discussion. It is also directly contradicted in page 10, line 9.

Page 10, line 25-26 is inconsistent with the statement on page 10, line 9, but in agreement with the previous discussion on page 9.

Reply: The reviewer is correct, page 10, line 9 contained conflicting information. We have corrected this mistake, so the sentence is revised as: *"…the Madeira and Tapajós basins are characterized by a large latitudinal gradient and, consequently, different ecosystems are present within these sub-basins. Hence, it is likely that, although on average water availability is not critical at the Tapajós and Madeira basins during the dry season, water limitation may occur in southern (drier) parts of these basins."*

We highlight that this paragraph is discussing spatial variabilities within basins, and not between basins. That is, because some of these basins have large latitudinal gradient, the southern (drier) portion of these basin may suffer from water limitation, while the northern (wetter) portions may not. This spatial variability is likely masked when assessing averaged ET for the entire basin.

Comment: Paragraph 4.2. The discussion is interesting and maybe it could be more clearly stated that ET and EVI don't seem to have any causal relationship. Therefore using EVI for modeling ET is not advisable.

Reply: We agree. We have included one more paragraph at section 4.2, to discuss the implications of the relationship between ET and EVI for modelling applications.

Comment: Page 11, line 5-6. There is no inter-annual analysis in this paper.

Reply: This is again a misunderstanding. The sentence is referring to "intra-annual variability" and not inter-annual.

Comment: Page 11, line 7. See comment for figure 5. This statement is not backed by data.

Reply: We removed this statement.

Comment: Page 12, lines 3-13. This discussion should be integrated in the interpretation of figure 4, as suggested above. PET is likely overestimated in Madeira, Purus and Tapajós as well as Solimões, as discussed on page 9, lines 26-30.

Reply: Although we appreciate the reviewer's suggestion, this is a debatable argument, as it is dependent on writing style. One may argue that any type of discussion should be kept separated from the results. For instance, guidelines from Columbia University clearly advises that the Results chapter should "not discuss the results or speculate as to why something happened". Similar

suggestion is repeated by guidelines published in an editorial in Nature Structural & Molecular Biology, and other sources.

http://www.nature.com/nsmb/journal/v17/n2/full/nsmb0210-139.html

http://www.columbia.edu/cu/biology/ug/research/paper.html

Minor comments

Equation 1, page 3 line 27, time is in a lowercase format by convention (needs to be corrected in the following line 28 as well)

Reply: Corrected.

Page 9, lines 16-17 should read P-dS (capital S)

Reply: Corrected.

If possible, clean the connection of the upper/lower parts on figure 1 (around 13S) to eliminate the artificial line/disconnect.

Reply: This is an artifact created during file conversion. The original figure doesn't have this line.

Figure 7. The values of Table 2 could be added to the figure, then the reader would not need to go back and forth from both of them.

Reply: In fact, the first version of this figure was exactly as requested, but after some tests, all authors agreed that the information is more clearly presented when the coefficients and R^2 are presented separately in a table.

Page 12, line 3 "therefore" means thus and should be spelled "therefore".

Reply: Fixed.