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Interactive comment on "Carbon Exchange in an Amazon Forest: from Hours to Years" *by* Matthew N. Hayek et al.

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(RC2) Hayek and others explore the net ecosystem exchange of carbon and its components over multiple years in a forest in the eastern Amazon. The simple model that they propose is interesting and challenges numerous assumptions regarding the seasonality of photosynthesis. That being said, some recent manuscripts by Wu et al. written by many of the authors (see also Kiew et al. doi.org/10.1016/j.agrformet.2017.10.022) indicate a strong vapor pressure deficit limitation to GPP that could (potentially) add quite a bit to the present manuscript given that highly statistical and empirical models are difficult to extrapolate. Investigating relationships between model residuals and VPD (or perhaps soil moisture although the authors are right in noting that its role is often over-simulated, especially given difficulties in measuring soil moisture at depth) would point

toward mechanisms that other models could benefit from. Addressing the following minor comments would in my opinion further improve an interesting manuscript.

(AC2) We agree that VPD can potentially exert a strong influence on NEE at timescales discussed in this manuscript. We actually included this variable in an early version of the analysis and manuscript, originally excluding it from this analysis. We incorporated some of these results back into our revised manuscript for reasons explained below.

We ran a monthly BIC for model selection on the annual average fit of our model and others on a monthly timescale, including models that had representations of the influence of VPD and diffuse radiation upon LUE, both together and separately. The model selection method accepted these variables for the hourly model fit, but rejected them for the monthly fit, implying that the additional independent variables did not add explanatory power at monthly and longer timescales. We now include this information in the Methods Section 2.6 and present the BIC scores Results Section 3.4.2.

These results are consistent with the results of the Wu et al. (2016b) manuscript to which the reviewer refers. Our conclusions were that many of these exogenous variables add explanatory power (in that case, to GEP) at hourly and daily timescales, but on timescales of months or longer, they become increasingly outweighed in effect size and statistical significance endogenous ecosystem changes. We therefore included a reflection on these results in the hourly and seasonal NEE Discussion Section 4.1 of our revised manuscript.

Furthermore, regarding interannual variability, VPD and diffuse radiation did not explain the 2002 anomaly, the subject of our discussion regarding legacy effects of the 1998 drought. In the BIC-rejected model containing both VPD and CI, the positive NEE/GEE anomaly remained. The difference between the 2002 model-data annual residual NEE from this VPD-containing model and our model was statistically insignificant. We also examined whether VPD or diffuse radiation were anomalously high or low, respectively, in 2002, causing decreased LUE and leading to lower GEP. Both annual mean VPD and diffuse radiation in 2002 lied within their decadal range.

Per the reviewer's suggestion to include a discussion of these variables for additional mechanistic insight, we discuss the anomaly analysis from our higher-parameter VPD and diffuse radiation model in Discussion Section 4.2 of our revised manuscript, and demonstrate that these variables did not significantly change the 2002 positive anomaly in NEE/GEE in our newly added supplemental Fig. S3.

(RC2) The passages on lines 33-35 in the Abstract are self-contradictory. Please reconcile.

(AC2) We removed reference to the 2005 and 2010 droughts specifically, which did not affect this site, and clarify in lines 31-32 that the 1998 drought did affect this site. We discuss the lack of impacts of the 2005 and 2010 droughts upon this site in the Discussion Section 4.2.1.

(RC2) The intro to line 37 in the paper is a disappointment given the Amazon's central role in global heat and moisture transport and global climate teleconnections. The climate system is about energy, not just carbon. Please re-write.

(AC2)We agree with the reviewer that energy and matter are both exchanged in large quantities by Amazon forests. We revised the introductory sentence to reflect this.

(RC2) The Introduction is otherwise well-written and nicely justified.

(AC2)We thank the reviewer for this assessment. We believe our introduction has been made even stronger by incorporating feedback from RC1.

(RC2) It would help the reader to justify the following passage using data on line 112-3: However, the interannual variability and trend remained the same regardless of the choice of u*Th

(AC2)We agree that this tendency in the data is important to highlight. We now refer the reader to Saleska et al., (2003), where this tendency for the variability and trend

to not be affected by the choice of u* filter can be seen for the first three full years of carbon exchange data for our site.

(RC2) Note inconsistencies in italicizations between equations and text for example in lines 165-6.

(AC2)We thank the reviewer for highlighting this discrepancy and have corrected it.

(RC2) 192 and elsewhere: add a space between the number and the unit (in this case mm). See https://physics.nist.gov/cuu/Units/checklist.html

(AC2) We have corrected this mistake.

(RC2) In paragraph 188, the definition of the dry season was a bit curious with less than 50 mm per 'half-month' of 3 or more 'semi-monthly' periods with low precipitation. Is this a running 'half-month'? Is the dry versus wet season in the Amazon not more consistently defined for people to extend the line of reasoning forwarded by this manuscript to other regions?

(AC2) Our definition of 50 mm per half-month is consistent with previous work on Amazon forest seasonal variability, which have collectively defined the dry season as 100 mm/month. We selected half-months to get higher resolution for the seasonal onset. Our definition of the wet season onset was unique but still consistent with the common 100 mm month-1 definition. We selected this definition because it allows for sporadic dry season downpour and ensures that there is not more than one dry season per year. Seasonality in tropical forests is typically defined by the mean seasonal cycle, whereas we were interested in interannual variability in the onset of the wet and dry seasons among various years. We added an additional line in the Methods section to explain that these half-month sums of rainfall were consistent with the common literature definition of full-month sums and provide reference.

(RC2) Line 204/205 needs a reference. Many modeling assumptions like this could benefit from more references to help the reader understand the decisions that went into model selection.

(AC2) We added a reference to the Wu et al. GCB manuscript this line and clarified that its inclusion was insignificant after controlling for other variables in the model.

(RC2) I am very surprised that the nice manuscript by Wu et al. (https://doi.org/10.1111/gcb.13509) is not cited in the present manuscript, particularly given their findings regarding diffuse radiation and vapor pressure deficit as important controls over GEP and of course the rather large overlap in authorship between this paper and the present manuscript.

(AC2) See our response to the first RC2 comment.

(RC2) I agree on line 218 that GEE 'represents the lowest-parameter approximation of a direct measurement, but a brief explanation for readers less familiar with eddy covariance (or readers who use the eddy covariance technique but are less familiar with its limitations) would be helpful. Qualifiers like 'strong' on line 225 and elsewhere can be avoided (and on that note of course NEE has a strong diurnal cycle). 'precisely quantified' on 369 is another example. And 'surprising' on 428. It may not have been a surprise to the forest.

(AC2) Per the reviewer's suggestions, we changed these lines by omitting these imprecise qualifiers and provide additional quantitative information for the GEE estimate and the reversal of the trend in annual NEE.

(RC2) On 290 do not use the * for multiplication as shorthand, this means complex conjugate (see also Fig. 5).

(AC2) We thank the reviewer for pointing out this mistake. We removed the parameter value from this line and corrected it in the caption of Fig. 5.

(RC2) The material on line 312 doesn't belong in a supplement in my opinion as the seasonal patterns of RE and GEE are important to the modeling effort.

(AC2) We agree that the seasonal patterns of carbon exchange are relevant to modeling efforts. This figure, however, concerns the interannual variability, and we believe that much of the information that it contains is already summarized aptly by figure 7b. We included this figure in the supplement for readers who were additionally curious about how annually averaged model-data residuals compare to the range in annual means for both the data and the model.

(RC2) 'best of a statistical model's ability' on line 324 is colloquial and probably doesn't hold for any scientific manuscript of reasonable length.

(AC2) We removed this phrase from the sentence.

(RC2) 339 and elsewhere: did 2002 have anomalously high VPD? (see also the paragraph beginning line 436).

(AC2) For the material in these sections, we now include discussion of VPD in Discussion Section 4.2 per the reviewer's suggestions in the first RC2 reviewer comment.

(RC2) Why is 'Fig. 2b' bolded on line 356?

(AC2) We corrected this mistake.

(RC2) 382: could it be shown that the hypothetical model would not add explanatory power or is this just assumed?

(AC2) We ran a monthly BIC for model selection on the annual average fit of our model and a spate of others, including a model that had a higher-parameter representation of phenology. The BIC score was in fact higher (less negative) for this model, implying that the additional parameters did not add explanatory power. We left this part of the analysis out of our methods, results, and discussion for the sake of brevity, being extraneous to the more relevant results. We changed this section, simplifying the discussion of phenology so as not to allude to this alternate parameterization, stating "Our single mid-year parameter simplistically up-shifts the trough in a more continuous seasonal oscillation between low and high LUE (Fig. 5) because we lacked independent variables explaining the seasonal oscillation."

(RC2) What is Wu et al. 2016a? This is not in the references.

(AC2) We corrected this inconsistency. Wu et al. 2016a and Wu et al. 2016b are now separate references.

(RC2) Regarding the 2002, is it possible that disturbance due to tower construction may have impacted NEE? I've seen results from a few towers where there seems to be some initial transient effect on C fluxes, not that the tower wasn't constructed carefully.

(AC2) We agree with the reviewer that we cannot rule out this or other measurement artifacts, such as those caused by tower construction creating a lower initial flux footprint than that assumed in typical eddy covariance measurement systems. We provide this important caveat in Section 4.2 of the revised manuscript, but qualify it by additionally clarifying that tower construction was completed almost a year before the measurements we used, with preliminary data collection occurring during 2001 (Saleska et al., 2003), potentially allowing time for transient effects to equilibrate.

(RC2) Table 1: uncertainty estimates should be presented with parameter estimates. (see also Table 2).

(AC2) We now present 95% confidence intervals alongside mean parameter estimates in Tables 1 and 2.

(RC2) Figure 1: I can't help but be surprised that a forest can continuously lose C to the atmosphere, but I've seen it in other tropical forests as well when measured using the eddy covariance technique. Per earlier work by the author and team, I wonder if ustar filters are appropriate for tropical forests although trying alternate filters like sigma_w (see papers by Jocher et al.) don't seem to change things in my experience.

(AC2) We found that similar filters that are turbulence-based indeed did not change our results, and in fact their use suggested that even more C was lost to the atmosphere when we used that approach. We included results from our alterative nighttime bias

correction in Fig S2. We now include a reference to Fig. S2 in the caption of Fig. 1.

(RC2) Avoid red (or red-ish) and green together in Fig. 7b.

(AC2) We changed these colors to be monochromatic in black and gray, respectively.

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