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Biogeosciences Discussions

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

Anonymous Referee #2

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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 oversimulated, 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.

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

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.

The Introduction is otherwise well-written and nicely justified.

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

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

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

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?

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.

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.

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.

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

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.

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

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

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

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

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

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.

Table 1: uncertainty estimates should be presented with parameter estimates. (see

also Table 2).

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.

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

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