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Interactive comment on "Climate seasonality limits carbon assimilation and storage in tropical forests" *by* Fabien H. Wagner et al.

Anonymous Referee #1

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This manuscript explores the seasonal correlation of carbon assimilation (estimated using MODIS EVI), above-ground wood productivity and litter productivity. The originality of this work stems in the large dataset compilation made by the authors, which includes data from 89 tropical sites throughout the world. The drivers of the seasonal dynamics of photosynthesis and structural growth has been studied separately in the past, but their joint analysis at large scale is a major novelty of this work. I have no doubt that the focus of this study is of general interest. However, I felt that an additional effort of clarification and justification would benefit the quality of the paper.

General comments

The word "storage" in the title, is in my opinion confusing as it could refer to the non-structural carbohydrate tree compartment. I suggest to consider another word (e.g.,

"above-ground growth").

The authors used statistical analyses that seems overly complex. For example, in order to demonstrate that wood productivity changes are mostly related to seasonal precipitations, the authors estimated simple linear models for each variables, then performed a McNemar test on the resulting contingency tables, before performing a cluster analysis on the table of p-values of the McNemar test. My general feeling is that this complexity might be required (for example in order to deal with collinearity issues among covariates) but must be better justify and better described (see detailed comments).

The logical link among the last three sentences of the first paragraph is not obvious to me. Brienen et al., as well as van der Sleen et al., used annually or multi-annually resolved datasets without explicit reference to the seasonal dynamic of the carbon cycle. I agree that this seasonal dynamic may be key to understanding some of the results reported by the cited authors: e.g., the increasing trend of tree mortality found in Brienen et al. may well be explained by the seasonal dynamic of soil water stress and leaf water potential (e.g., Rowland et al., Nature, 2015), or the seasonal dynamic of non-structural carbon reserve (e.g., Dickman et al., Plant, Cell & Env, 2015). This part of the introduction needs, in my opinion, to be rewritten in order to justify the importance of a better understanding of the seasonal drivers of the carbon cycle. More generally, I felt that this introduction needs to include a broader overview of what is already known about the seasonal dependencies of the carbon cycle to climate and internal factors. Some very interesting results have been obtained (some of them authored by the authors of the present study) regarding the determinism of the seasonal growth of the different tree compartments (wood, leaf, fine root, reserve), as well as the dependencies of photosynthesis in a tropical context. A presentation of these previous works would allow the reader to better understand the novelty and the limits of the present study.

The enhanced vegetation index used by the authors in this study is claimed to be a proxy of "the canopy photosynthetic capacity". Among others, a recent study cited by the authors (Guan et al., 2015, Nature Geosc.) indeed used EVI as "a proxy for vegetation greenness and photosynthetic potential", which is supported by the strong correlation between EVI and satellite-based chlorophyll fluorescence. But EVI is also known to "include information on forest canopy structure" (Guan et al., 2015, Nature Geosc.). I felt that the complexity of the EVI signal is not emphasized enough in the present paper, as the authors refer to EVI as a "proxy of leaf production", that is, only forest canopy structure. For example, the author have to assume that some big trees at light-limited sites shed leaves because of high evaporative demand, in order to explain the decoupling between EVI and litterfall. It is indeed plausible, but the authors should provide some references to support this assumption, otherwise, they should discuss as well the possibility that – for the range of LAI explored here – EVI changes reflect actually mainly the changes in leaf photosynthesis activity (gC/m2leaf, independently of the total leaf biomass and litterfall). Could you explain why you discarded this latter possibility? I think that your dataset (EVI + litterfall at numerous sites) is very much suited to explore what is actually measured by the EVI.

The authors assumed that a fraction of the 89 studied sites are light-limited, based on reported higher temperatures during dry than during wet season at these sites. Although I agree that light limitation at wet sites is in line with our knowledge of photosynthesis, I felt that this assumption, that is key in this paper, deserve further justification. Indeed, the assumption that temperature actually reflects "solar energy available for the plants" is not supported by references. This is annoying, because temperature is closely link to the water evaporative demand, which is a component of drought. I have the feeling that the correlation between normalized precipitation and normalized EVI at light-limited sites may be significantly negative, which may be in line with the author's assumption (given that precipitation occurs only with a cloud cover). Would you obtain patterns similar to Fig. 5c with a variable more directly related to light availability, such as standardized cloud cover?

Specific comments

P.5. - L.13. "discrepancies" is confusing here, as we do not know which discrepancies the authors are referring to (temporal discrepancies, or discrepancies between biomass and photosynthesis). I suggest using another word, e.g. "patterns".

P.6. - L.11. The tables are not cited in ascending order (Table 2 is cited for the first time before Table 1). The figures are not cited in ascending order either. Please, correct this.

P.6. - L.22:23. "For each tree…" This sentence is unnecessary (and in my opinion, confusing), as the whole process is explained in details in the subsequent sentences. Furthermore, please explain in which cases you deleted the increment, and in which cases you corrected it.

P.7. - L.3. Please, mention here the temporal and spatial resolution of the MODIS product.

P.7. Please, better justify the processing of EVI data (e.g., why did you use a square of 40km? Why did you average pixels surrounding the sites, instead of simply using the values of the pixel sites?).

P.8. - L.5:6. This formulation "normalized by their site's annual mean values and standard deviation" is confusing. Please, give more details about the standardization methodology.

P.9. - L.17. If I am not mistaken, "the predictive model of wood productivity by precipitation" has never been presented before. Consequently, we do not know what the authors are referring to. Please, correct this sentence, and cite the Table 4 in the Results section.

P.12. - L.32. "From the climatic point of view" is not proper English.

P.14. - L.16. I do not see the point to referring to the genetic loci names here. Please, explain in more details how and why this information is relevant.

P.15. - L.5:6. I do not understand this sentence. What is the difference between carbon assimilation and photosynthetic capacity seasonal pattern? Please rephrase.

P.15. - L.7. This last sentence is confusing to me. What is a “direct limitation of canopy photosynthetic capacity” compare to “a reduction of canopy photosynthetic capacity in the dry season”. Please rephrase.

Figure 5. Why do the dash lines represent the relationship between climate variable and modelled EVI, rather than observed EVI, in line with the data depicted with dots? The statistic info of the different regression lines should be provided.

Figure 7. Please, rewrite this caption. It does not accurately describe the figure. In my opinion the cross correlation plot, especially Figure 10, do a poor job in illustrating the author’s results. For example, I do not understand how Fig. 10a shows that “EVI seasonality is well associated with aboveground wood production for water-limited forests”. If I assumed that the Y-axis is actually the “number of sites with a significant correlation”, which is not mentioned by the authors, I could evaluate this statement by comparing the number of significant sites against the total number of sites. . .which is not straightforward. The direct information given by these plots is whether of not the light and water-limited sites have a similar time-lags in their correlation. This information is however not discussed by the authors.

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