Biogeosciences Discussions

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Comment on bg-2021-30

Anonymous Referee #2

Referee comment on "Drought effects on leaf fall, leaf flushing and stem growth in the Amazon forest: reconciling remote sensing data and field observations" by Thomas Janssen et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2021-30-RC2, 2021

General comments

Janssen et al's manuscript brings a very good contribution to understanding tropical forest responses to multidecadal climatic variation across the Amazon basin by combining available *in situ* measurements of components of forest productivity (long-term records of stem growth and leaf litterfall) and remote sensing products under a robust modelling approach. Results show significant long-term trends of decreasing stem growth, and a less strong increase of leaf litterfall in the Amazon basin since the early 1980s.

Overall, the manuscript is very well written, and I have no issues on number and quality of figures/ tables, or language. I have some specific questions on the methods as described below, and I hope solving these questions will improve the manuscript.

Although the authors compiled data from the literature from sites across the neotropics, the evaluated effects of drought on modelled leaf fall, leaf flushing and stem growth are centered in the Amazon basin; therefore, I believe the manuscript title is misleading the readers about drought effects on "Neotropical forest". Please consider this aspect.

Specific comments

The observational dataset on stem growth and litterfall was not available for review, but I think it is valid to describe how data on stem growth and litterfall are distributed along the period 1982-2019; is it uniform? If not (more data on the first two decades when compared to more recent years), how did this discrepancy (or unbalance) affect the predicted values? Was that issue contemplated during the machine learning process (L230-243)?

Observational data was compiled for the entire Neotropics (as shown in Fig 2a, b, and described in the methods), so why did the subsequent core analyses have "cropped" a specific area centered in the Amazon basin (15oN – 20oS), excluding central America, Atlantic forest and part of the Brazilian caatinga and Cerrado? I could not find any explanation in the methods on this topic.

Figure 8: What is the interpretation of the authors on the seasonal anomalies (especially Fig. 8c) shown in middle 2006? Those values look significantly higher than the ones found for the 2005 drought period (gray area in the figure).

On the conclusion (L580-583), it is unclear what is the opinion of the authors on the inconsistencies between the trends obtained here using modelled time series associated plus remote sensing and the trends observed using long-term inventory plots. Are remote sensing and ground-based trends reconcilable? Plot networks are not free from spatial bias, as correctly pointed out by the authors, so are plot-based carbon sequestration overestimated?

L583-585: actually, tree mortality rates can be even higher few years after drought (not during the droughts) as drought-induced mortality is not instantaneous

Technical corrections

L301-304: How did "biomass production" was estimated in Fig 2c and Fig 9c?

L345-348: some readers may not know where Cerrado and Caaating regions are located in the map; please use geo-locators, marks, or arrows to point out those areas

Figure 8 was mentioned in the text before figures 6 and 7; please review

Fig 9: please provide a source (reference) for the multivariate ENSON index (or include it Table 1)

Please, check if all references are correctly cited according to the Journal's recommendation; some of them looks incomplete, for instance, L972

P39, L960: hyperlink for the publication is broken, it looks like missing letter “r” in the end