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Comment on bg-2021-30

Anonymous Referee #1

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-RC1, 2021

General comments

Understanding how tropical forest trees respond to extreme events is critical if we want to quantify the carbon cycle of the terrestrial biosphere to ongoing climate change. In this study, Janssen and co-authors use a collection of remote sensing and gridded environmental variables to develop machine learning models to predict leaf liter fall and stem growth across the tropical and sub-tropical areas of the Americas. The authors used data retrieved from the literature to train and test the models, and applied the model over long time series to identify how leaf production and stem growth during Amazonian droughts and to explore the long-term trends, and found increased leaf production but reduced stem growth during droughts, and a small but significant long-term decline in stem growth.

Overall, this is a very well-written paper with a mostly clear and interesting analysis that integrates historic observations with multiple remote sensing products. It has a potential to become an important contribution to advance our understanding of two critical processes in forest dynamics (leaf litter fall and stem growth) with limited information across the tropics beyond intensively studied sites. However, I have some questions and points, mostly regarding the machine learning methods, that may deserve additional clarification and potential development to assess and improve the model robustness (see below).

First, unless I missed it, the authors did not explain how they addressed the issue of some time series being shorter than others. For example, most meteorological drivers extend from 1981 to 2019, whilst MODIS EVI is not available before 2000. What was exactly done in the model for the periods in which less data were available?

In addition, the authors used a single time for a few variables, and presumably assumed the values constant (if not, please clarify). I understand that some data sets may be simply not available for more than one time (SLA) or their time series may be uncertain (biomass), but assuming these quantities constant is a substantial simplification for the time span of this study. This is especially true for deforested areas and forest edges, which may rapidly change. There are a few data sets that could work as proxies for the forest dynamics (for example, Hansen et al. 2013 or Song et al. 2018 for tree cover).

The authors used data digitised from published figures, which is truly a heroic effort, yet the sample is spatially very limited, and this makes me wonder how robust the model is for spatial extrapolation. When the authors tested the model performance, they retained 60% of data from each site for training, and used the remaining 40% of the data for cross-validation. This approach tests how well the model performs in each site, but it does not tell the accuracy of the model predictions in grid cells with no data. I suggest the authors to perform an additional test in which the training/testing data are split by sites (i.e., no data from the test sites are provided to the machine learning during training). This will be an imperfect assessment as some regions do not have any data (e.g., stem growth in the Caatinga region), but at least it may indicate some of the model limitations more clearly.

Finally, I can see the maps generated in this study to be used by many other studies, and I think the authors could think of ways of spatially quantifying the uncertainties associated with the predicted quantities. For example, I imagine that the XGBoost approach generates multiple predictions (from each regression tree) that could be used to estimate the uncertainties for each time and location. There are also uncertainties in the training data sets that can be incorporated into the total uncertainty (somewhat similar to Chave et al. 2004), although this may not be feasible here because of the lack of uncertainty from the digitised observations.

Specific comments

Domain: The authors used data from 30°S to 30°N (L. 115), but most figures show the results between 20°S and 20°N, sometimes including Eastern South America and sometimes excluding it. Considering that most of the data are from tropical forests and the discussion is focused on the Amazon, I wonder if the domain should be consistently defined as the Amazon or moist tropical forests only.

Section 2.3. I think this section needs more information about the processing of the remote sensing data. For example, MODIS data sets come with multiple quality flags, and the results can be highly influenced by the choices on how data were filtered and aggregated. I suggest explaining this processing either in the section or as a supporting information.

Figure 1. The model predictions show a pattern that is rather common in machine learning regressions (overestimation at the lower range, and underestimation at the upper range). In other words, the tails are biased. This presumably affects the predictions at the extreme events (droughts), when one would expect litter fall and stem growth to be anomalous too. I think this limitation must be highlighted when presenting and discussing the results.

Section 4.1. I am somewhat confused with the mechanisms the authors are describing in this section, and they seem contradictory as presented now. The authors mention in the first paragraph that plants may shed leaves to maintain the xylem integrity, but then in the second paragraph they show large positive anomalies in the early months of the drought. Wouldn't this greening lead to higher transpiration rates and higher risk?

L538. Presumably, these trends were also present in the environment where measurements were carried out. In this case, either the ERA5 trends are stronger than observed, the XGBoost model exaggerated the contribution of climate to stem growth and leaf litter fall, or the climate trends were stronger in areas where the authors did not have any training data. I think the interpretation of the discrepancy between the model and the reference data must be discussed more clearly.

Minor comments

L23. Briefly mention what are the other geospatial datasets.

L47. Both wet and dry extremes are becoming more recurrent in the Amazon (Gloor et al. 2013).

L69. There have been also studies suggesting that the apparent green-up could be an artifact caused by sun sensor geometry (Morton et al. 2014).

L199. State somewhere in this paragraph the time span of the SIF data.

L205. Also indicate the time span of the VOD data.

L232. Is there any justification for the 3-month window?

L323. At least from Fig. 3, I do not see consistent increase in leaf flushing across the boxed region, I see both positive and negative anomalies.

L328. Why not showing air temperature? This could appear in the Supporting Information.

L336. This could also appear in the Supporting Information.

L347. Use "drier" instead of "dryer" (check for additional occurrences)

L347. The EVI response in central Amazon does not look like a consistent green-up. There is a strong negative anomaly band in the central part of the box. This seems to coincide with an area thought to more susceptible to droughts (Hirota et al. 2011; Longo et al. 2018), and that was also affected by significant fires during the 2015-2016 drought (Aragão et al. 2018; Withey et al. 2018).

L385. There are some significant positive anomalies of VOD along the rivers. It is hard to tell but it looks like the authors did mask inland (permanent) water bodies (if not, I suggest doing it). So it is a bit puzzling to me why the areas near the rivers would show such strong increase in 2005, when river levels were so low (Tomasella et al. 2011).

L391. From Fig. 8c, it looks like 2006 was more extreme than 2005 itself. Also, from Fig. 8b, it looks like EVI was already very positively anomalous even before the drought.

L418. Did the authors consider dividing the values by the monthly standard deviation? It may help to remove the seasonal variation of the interannual variability still visible in Fig. 9.

L436 (and in other places throughout the text). El Niño is the warm phase of the El Niño Southern Oscillation (ENSO). So when referring to the climate pattern in general (e.g. line 436) use ENSO. When referring to the warm phase (line 437, 438), use El Niño instead. And use this notation consistently throughout the text.

L458. Although there is a large variability in drought strategies in the Amazon, and there are trees that can keep transpiration at similar levels during dry periods (Maréchaux et al. 2018; Oliveira et al. 2019).

L479. "Attributed" instead of "contributed".

L516. Could drought duration have played a role here?

Table 1. I think the table should include all the data used in the XGBoost models, including the remote sensing and the derived quantities. Also, indicate which version of SoilGrids was used, and which soil layers were used (or if all depths were used). For ERA5, there is now a peer-reviewed publication too (Hersbach et al. 2020).

Figures 2–4, 6–7. I suggest using more intuitive palettes (viridis, magma for the absolute plots, divergent palettes with white near zero for anomalies). Also red-green scales can be difficult for some people.

References

Aragão, L. E. O. C., et al.: 21st Century drought-related fires counteract the decline of Amazon deforestation carbon emissions, Nature Comm., 9, 536, doi:10.1038/s41467-017-02771-y, 2018.

Chave, J., et al.: Error propagation and scaling for tropical forest biomass estimates, Philos. Trans. R. Soc. B-Biol. Sci., 359, 409–420, doi:10.1098/rstb.2003.1425, 2004.

Gloor, M., et al.: Intensification of the Amazon hydrological cycle over the last two decades, Geophys. Res. Lett., 40, 1729–1733, doi:10.1002/grl.50377, 2013.

Hansen, M. C., et al.: High-Resolution Global Maps of 21st-Century Forest Cover Change, Science, 342, 850–853, doi:10.1126/science.1244693, 2013.

Hersbach, H., et al.: The ERA5 Global Reanalysis, Quart. J. Royal Meteorol. Soc., 146, 1999–2049, doi:10.1002/qj.3803, 2020.

Hirota, M., et al.: Global Resilience of Tropical Forest and Savanna to Critical Transitions, Science, 334, 232–235, doi:10.1126/science.1210657, 2011.

Longo, M., et al.: Ecosystem heterogeneity and diversity mitigate Amazon forest resilience to frequent extreme droughts, New Phytol., 219, 914–931, doi:10.1111/nph.15185, 2018.

Maréchaux, I., et al.: Dry-season decline in tree sapflux is correlated with leaf turgor loss point in a tropical rainforest, Funct. Ecol., 32, 2285–2297, doi:10.1111/1365-2435.13188, 2018.

Morton, D. C., et al.: Amazon forests maintain consistent canopy structure and greenness during the dry season, Nature, 506, 221–224, doi:10.1038/nature13006, 2014.

Oliveira, R. S., et al.: Embolism resistance drives the distribution of Amazonian rainforest tree species along hydro-topographic gradients, New Phytol., 221, 1457–1465, doi:10.1111/nph.15463, 2019.

Song, X.-P., et al.: Global land change from 1982 to 2016, Nature, 560, 639–643, doi:10.1038/s41586-018-0411-9, 2018.

Tomasella, J., et al.: The droughts of 1996-1997 and 2004-2005 in Amazonia: hydrological response in the river main-stem, Hydrol. Process., 25, 1228–1242, doi:10.1002/hyp.7889, 2011.

Withey, K., et al.: Quantifying immediate carbon emissions from El Nio-mediated wildfires in humid tropical forests, Philos. Trans. R. Soc. B-Biol. Sci., 373, 20170 312, doi:10.1098/rstb.2017.0312, 2018.