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Biogeosciences Discussions

Interactive comment on "Leaf Area Index Changes Explain GPP Variation across an Amazon Drought Stress Gradient" *by* Sophie Flack-Prain et al.

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We thank the reviewer for their time and effort and very helpful comments. Detailed answers to the comments are provided below. Author responses are denoted by AR.

Review of Leaf area index changes explain GPP variation across an Amazon drought stress gradient by Flack-Prain et al, Biogeosciences, 2019. This paper describes how GPP changes along a gradient are explained by direct and indirect effects of climate forcing in Amazonia. The direct forcings include physiological responses while indirect responses include ecosystem structural and leaf trait responses. The authors use observations and a calibrated soil, plant, atmosphere model to single out the different responses. They find that indirect responses dominate the explanation of the spatial

variation, whereas seasonal variation was dominated by response to global radiation, the strength of which depends on the level of drought. The paper is in general well written, well structured, concise and to the point. There are a few point that need attention:

- In a few instances, which I will describe below, I found the formulation of sentences vague.

AR 1. Edited in accordance with specific comments

- In my opinion research question 3 adds little value to the paper and the corresponding results are relatively shallow relative to the existing literature. The results are quite obvious. I suggest removing this rq and the corresponding results. It will make the paper sharper and more to the point.

AR 2. We take on board the reviewer's point with regards to RQ3. However, the reviewer's subsequent comment regarding discussion on the temporal scale of responses prompted additions to the manuscript which we feel highlights the importance of RQ3s inclusion (see AR 3).

- I miss a discussion on the temporal scale of the responses. The authors use 'multiple' (2) years of forcing data. They find that indirect responses dominate. I understand that the paper describes equilibrium responses to an existing drought stress gradient. Still indirect responses probably need some time to develop, while droughts are often intermittent. If I do not fully understand how the authors see this, it may indicate the need to discuss this issue explicitly.

AR 3. We thank the reviewer for highlighting an important issue around the definition of drought stress. In the presented study we focused on seasonal drought stress, and compared GPP drivers across seasons and across a gradient in seasonal drought stress. In the original version of the manuscript a clear distinction between seasonal drought stress (our focus) and drought events (not addressed in the manuscript) was

not made. By defining drought stress in the context of our study early on we hope to ensure our references to drought stress are not ambiguous. The temporal scale of responses can then be discussed by comparing model experiments 1 and 3. We appreciate that the comparison was somewhat limited in the original version of the manuscript and as such have expanded the discussion (on from line 516).

- The model methodology is explained insufficiently to allow for independent reproducibility of the results and for understanding what the authors really did.

AR 4. A new figure has been added to the supplementary material detailing the inputs of each model run used in the analysis. We hope this figure helps sufficiently explain our methodology. However, we would be happy to consider further changes if the reviewer could provide more information on which details they believe to be missing.

Specific comments

- Line 90. Please explain on what time scale this evidence is valid.

AR 5. We have now included detail on the relevant timescale. Evidence exists across sub-annual (Araujo-Murakami et al., 2014, Xu et al., 2016) and annual timescales (Brando et al., 2008, Meir et al., 2009).

- Line 90. This paragraph is rather qualitative, therefore vague. Please explain how strong the responses are.

AR 6. Leaf area index (LAI) typically decreases with increasing drought stress (Meir et al., 2015a, Grier and Running, 1977). Across the wet-dry tropical forest transition LAI declines on average $\sim$1.4 m2m-2 (Iio et al., 2014). Temporal changes in drought stress report a 21-26% decline in LAI following five years of drought onset (via throughfall exclusion at Tapajós National Forest, Pará, Brazil; Brando et al., 2008). With respect to near surface root mass, length and surface area growth declines with seasonal water deficit, the paper referenced (Metcalfe et al., 2008) does not provide estimates on the strength of the response, only that is it significant ($p<0.001$). However, from the figure

presented we can estimate that root mass, length and surface area growth decline by up to 75%, 65% and 25% respectively (approximated using figure data retrieval software). Quantitative details have now been added to this section.

- Line 104: understanding is limited. This is quite an empty sentence. Please make it more concrete by stating what understanding is missing exactly.

AR 7. On reflection this sentence repeated (but with less specifics) the earlier statement of "The relative importance of plant physiology, ecosystem structure, and trait composition responses in determining variation in GPP, remain largely unquantified in data-constrained analysis (Meir et al., 2015b)." We have therefore removed it.

- Line 127. We link . . . . Vague sentence. Additionally, 2 years is really the minimal number of multiple years. Couldn't you use a longer data set? This is relevant to how fast ecosystems respond to and recover from drought. How do you capture transient responses and how do you know those 2 years are representative for average (or not extreme) conditions?

AR 8. With respect to the reviewers comment on drought response and recovery please see AR 3. In response to the reviewers question about using a longer data set. We were limited by the length of the timeseries available across plots of different data streams and the need to keep the number of annual GPP estimates from each plot consistent for a balanced statistical analysis.

- Line 171. Please define MCWD precisely.

AR 9. The introductory methods section has been edited as follows, and the MCWD equation added to the supplementary material. MCWD is the maximum cumulative water deficit reached within a year. A water deficit estimate for each month is calculated as the difference between precipitation and transpiration (which ground measurements estimate at âĹij100 mm month$-$1, see Aragao et al. (2007)). Therefore, the forest is in water deficit if monthly precipitation falls below 100mm. Maximum cumulative water

deficit is calculated as the sum of sequential monthly water deficits (see supplementary material for equation).

- Line 208. How frequent were data gaps?

AR 10. The number of missing hourly field meteorological measurements across the timeseries varied from 2-40% across sites, whilst the frequency of gaps varied from 2-99 yr-1. Gaps less than 6 hours in length accounted for between 20-100% of total gaps across plots. Statistics on the frequency of data gaps has now been added to the manuscript.

- Line 222. . . .overestimation. . . please quantify.

AR 11. Pre-calibration, SPA soil moisture estimates were on average 11-68% higher than field measurements across plots. The difference between model and field soil moisture estimates increased significantly with MCWD (R2=0.69, p=0.04).

- Line 254. This sentence confused me initially, it sounds like you are only focussed on direct effects. Please rephrase.

AR 12. Rephrased to "Our aim was to isolate the direct effects of climate and soils (via physiological responses), and the indirect effects via ecosystem structure, and leaf traits, on simulated GPP." - Line 359. Please clarify how this can be seen in table 3.

AR 13. This was an error and should have referenced the preceding section ('3.4 LAI and Leaf Trait Trends along the MCWD gradient'). Now corrected.

- Section 3.5.2. This section is difficult to read because of the many numbers. Is presenting them in a table of figure an option?

AR 14. The authors have moved the site specific Δ values to Figure 6.

After these points have been addressed, I advise positively to publish this paper in Biogeosciences as it is a valuable contribution to better understanding spatial and temporal variations in GPP in Amazonia and the carbon cycle in general.

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