

## **Review of “The Response of the Amazon Ecosystem to the Photosynthetically Active Radiation Fields: Integrating Impacts of Biomass Burning Aerosol and Clouds in the NASA GEOS ESM”**

**by Bian et al.**

### **Summary:**

There has been a growing number of studies looking at the potential impact on land carbon uptake from increased availability of diffused radiation associated with aerosol particles. Although conceptually simple, this effect is hard to quantify accurately as complex couplings between different components of the Earth system are at play. An Earth System Modelling (ESM) approach appears as a natural framework for this kind of problem, yet only a handful of studies using ESMs has been published so far. This submission from Bian et al. is therefore timely as they used the results from simulations performed with the NASA GEOS-ESM to analyze the impact of biomass burning aerosols on the Amazon rainforest gross primary productivity. The diffuse light fertilization effect from aerosols is not only uncertain, but it is also buffered by clouds as those compete with aerosols for radiation. This is in a way similar to pre-industrial natural aerosols controlling the amplitude of the anthropogenic aerosol radiative forcing. The role of clouds on the aerosol diffuse light fertilization effect has not been properly explored before and Bian et al. provide a novel quantification for this modulating effect.

It's an interesting paper, easy to read, the structure is sound, and the supporting material is adequate. The work perfectly fits within the topics covered by ACP so I strongly support its publication after addressing those minor few points.

[We thank the reviewer for his/her good summary and comments. Please see our point-to-point reply below in blue.](#)

### **General Comments:**

1. How was the 7 years period selected? Surely the observational datasets used for the evaluation cover a longer range so it could have been an opportunity to extend the statistics.

[We intended to select years that were continuous and covered the periods of La Niña, normal years, and El Niño. This 7-year period \(2010-2016\) fit our](#)

selection standard according to the NOAA's Ocean Niño Index (ONI) anomaly (see Figure S1 and text lines 317-320).

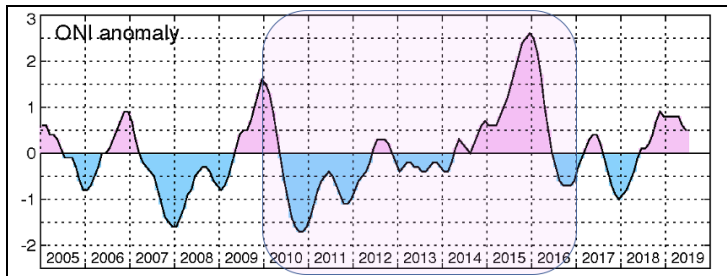


Figure S1. Ocean Niño Index (ONI) anomaly, Jan 2004-July 2019. Source: <https://origin.cpc.ncep.noaa.gov/>. El Niño or La Niña is identified when the ONI anomaly exceeds  $\pm 0.5^{\circ}\text{C}$  for at least five consecutive months. The study period (2010-2016) covers La Niña (2010-2011), normal (2012-2014), and El Niño years (2015-2016).

We also added the information of long-term BB OA emissions (i.e., 1997-2016) and long-term MERRA2 cloud fraction anomalies (i.e., 1995-2018) in Figure S2. The selected 7-year period represents well the long-term period in terms of the variation of BB emissions and cloud coverage. See lines 320-323.

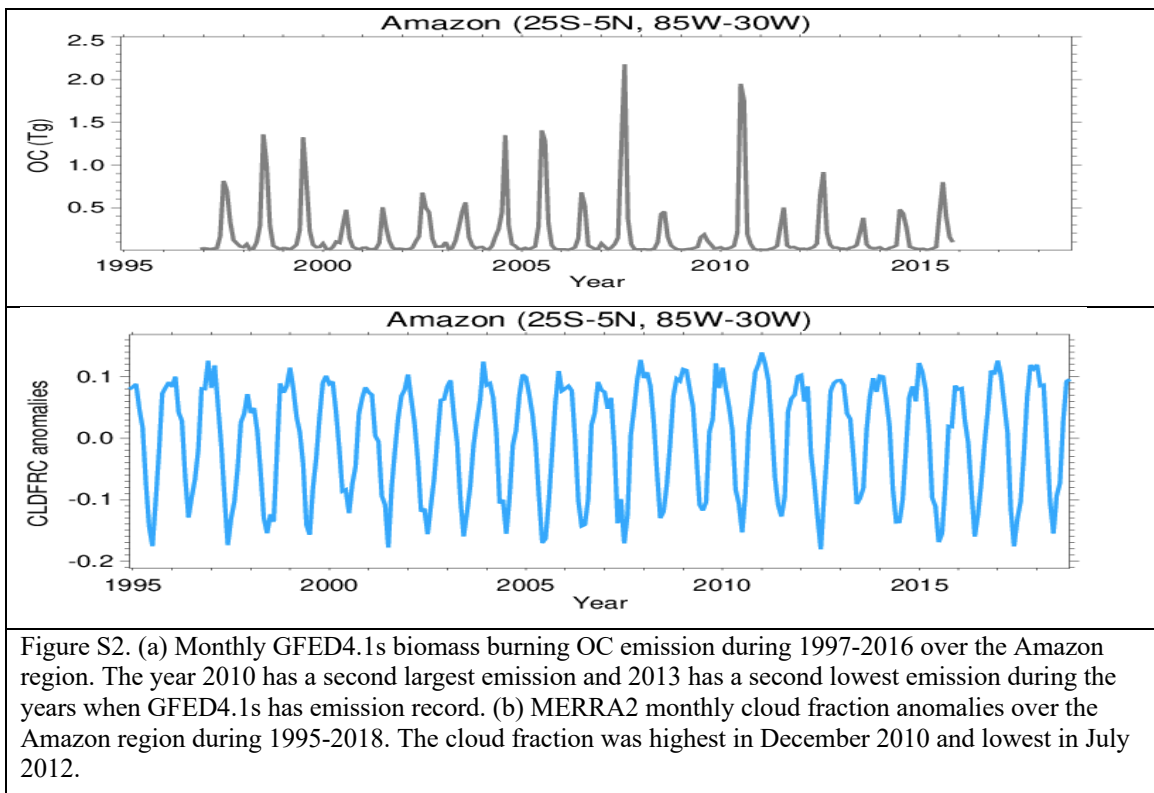
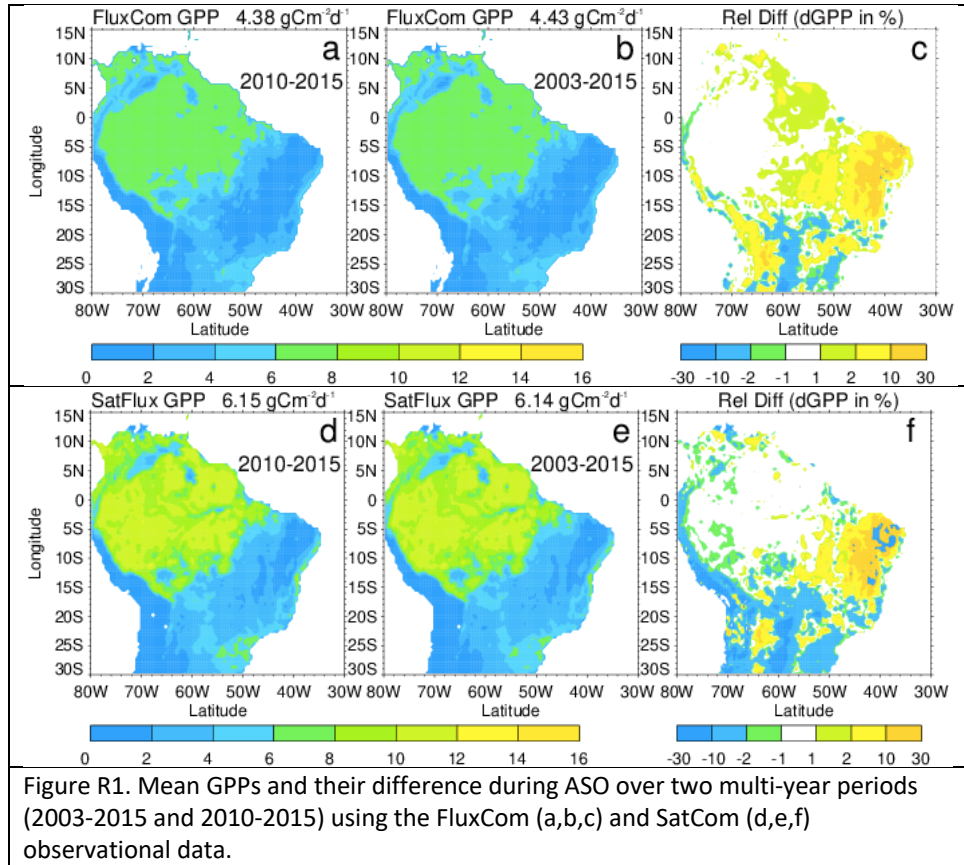


Figure S2. (a) Monthly GFED4.1s biomass burning OC emission during 1997-2016 over the Amazon region. The year 2010 has a second largest emission and 2013 has a second lowest emission during the years when GFED4.1s has emission record. (b) MERRA2 monthly cloud fraction anomalies over the Amazon region during 1995-2018. The cloud fraction was highest in December 2010 and lowest in July 2012.

To examine whether the 7-year analyses are valid over a longer period, we also added a comparison of mean GPPs and their difference during ASO over two

multi-year periods (2010-2015 and 2003-2015) using the FluxCom and FluxSat observational data (Figure R1). The longer period covers all the years when observational data are available. Compared to the perturbed GPP due to aerosol diffuse radiation fertilization effect (dGPP(aer)) shown in Figure 11, the large dGPP shown here (Figure R1) appears in the areas outside the effective regions of dGPP(aer) (Figure 11).



- I would be tempted to rename the “fertilizer effect” as the “fertilizing effect” as fertilizer and effect are both nouns and it sounds odd to me. I’m not a native English speaker thought.

Based on the comments of both reviewers, we now use “diffuse radiation fertilization effect” throughout the manuscript.

- How is the seasonality (not just August – September) of clouds in your simulations - e.g. ICTZ temporal/spatial position which would affect moisture availability - when compared against observations? Same question for the seasonality of GPP/NPP. Have you evaluated the simulated seasonal cycle against the two products mentioned in the manuscript (i.e. FluxCOM and FluxSat)? Those could be potentially be added to the supplementary material.

Based on the reviewer's suggestion, we have added Figures S5a-c and S7. Figure S5a-c, combining with Figure 3, shows seasonality of clouds from MODIS and GEOS during 2010-2016. The model has better performance during the period of Aug-Oct, which is the focus period of this study since Amazon fires occur periodically every year in this season. We added this discussion in lines 469-472. Figure S7 shows seasonality of GPP from FluxCOM, FluxSat and GEOS during 2010-2015. During all four seasons, regional FluxCom GPP is the lowest and FluxSat GPP is the highest. All observational and model GPPs show higher values during Nov-Apr than those during May-Oct. See lines 501-509.

4. The description of the representation of light interception by vegetated canopies (Line 235-237) could benefit from being described in a bit more details as this part of the model is central to the present study. Is it the same parameterization as the one in CLM4?

Yes. The light interception by vegetation in this study adopts the same parameterization as the one in CLM4. We modified the relevant description in lines 235-236 by explicitly indicating this point: "The light interception by vegetations in the GEOS Catchment-CN adopts the same parameterization as the one in CLM4. The photosynthesis and transpiration depend non-linearly on solar radiation. The canopy is assumed to consist of sunlit leaves and shaded leaves, and the DRPAR and DFPAR absorbed by the vegetation is apportioned to the sunlit and shaded leaves as described by Thornton and Zimmermann (2007). The prognostic carbon storages underlying the phenological variables are computed as a matter of course along with values of canopy conductance that reflect an explicit treatment of photosynthesis physics."

5. A bit off topic but I am curious as you mentioned photolysis and brown carbon developments in section 2. Are these two coupled? If so, is there any detectable impact on photolysis rates and consequently on ozone formation?

No. The newly developed brown carbon absorption has not been implemented in the GEOS online photolysis simulation that may impact ozone chemistry. The new stronger brown carbon absorption in UV spectrum has only been accounted for in the GEOS online simulation of radiation. The photolysis calculation that demos the concept of how aerosol and cloud impact on surface downward radiation (Figure 9) was conducted with a stand-alone FastJX model.

6. If I understand the experimental design correctly, the radiation driving the atmospheric model in callaer and cnobbaer for pair 2 (respectively allaer and nobbaer for pair 1) is the same, namely R1. Does that imply that there is

potentially an implicit accounting of BB aerosol semi-direct effects in cnobbaer (respectively nobbaer), meaning that any potential change in cloudiness when doing a Diff between callaer and cnobbaer won't be captured? Given that 2010 was a drought year with high BB emissions (see figure 10), could this lack of change in CF be meaningful and affect the slope of  $ddGPP/dAOD$ ?

Excellent point. Cloudiness (CF) and other relevant important meteorological fields have certain differences between the two pair simulations due to R1 associated with different aerosols or even between two simulations within a pair due to land ecosystem feedback. To examine whether these differences result in a meaningful impact on our conclusion, we compared CF, Skin temperature, and soil moisture simulated by all simulations (allaer and nobbaer in pair 1 and callaer and cnobbaer in pair2) over our study period, see Figures S3-4. The very small changes of these fields among the four simulations shown in the figures support our approach. The similar atmospheric fields stem from the model feature we adopted in this study. In order to focus strictly on the ecosystem response to the into-ecosystem light perturbed by biomass burning aerosols, we use a Replay mode configuration in simulations. In Replay mode, every six hours, the model atmospheric dynamic state (winds, pressure, temperature, and humidity) is set to the balanced state provided by the Modern-Era Retrospective analysis for Research and Applications, Version 2 (MERRA2) and then a six-hour forecast is performed until the next analysis is available. MERRA2 is the assimilation system that enable assimilation of modern hyperspectral radiance and microwave observations, ozone profiles observations, along with GPS-Radio Occultation datasets. Simulation with Replay is important in this study with two advantages: 1. Nudges GEOS dynamic fields to MERRA2 reanalysis ensuring atmospheric conditions of the four simulations are close to each other, therefore, resulting in more focus on the study of into-ecosystem radiative impact, 2. Ensures the observational-constrained meteorological fields used in our study. Please see lines 309-317.

7. Another aspect of the experiment design that is not clear to me is whether the inputs (not just radiation) from the atmospheric model to Catchement-CN in nobbaer and cnobbaer are from the atmosphere that has experienced R1 or from the atmosphere that has experienced R2. If the former, as I understand it, it would mean that the vegetation only 'feels' the aerosols via the change in dir/diff radiation but not via other changes in the energy balance that aerosols also introduce. Can you clarify?

The reviewer is right that all atmospheric calculations use R1, see Table 1. The experiments in this study were intentionally designed in this way so that the vegetation only 'feels' the aerosols via the change in dir/diff radiation but not via

other changes in the energy balance that aerosols also introduce. Future study may extend to include these other changes. We added this clarification in lines 366-370.

### Specific Comments:

1. Line 29, change “the impact” to “this impact”.

Done.

2. Line 32, change “call” to “called”. I would however argue that the light fertilization effect is only one of the impacts (plural, as in the manuscripts) resulting from aerosol radiative effects (e.g. less surface radiation affects the energy balance hence has an impact on vegetation productivity too). Maybe this sentence could be slightly reworded.

The sentence has been changed to “The direct radiative impact of biomass burning aerosols on ecosystem productivity—called here the aerosol diffuse radiation fertilization effect—is found to increase Amazonian Gross Primary Production (GPP) by 2.6% via a 3.8% increase in diffuse PAR (DFPAR) despite a 5.4% decrease in direct PAR (DRPAR) on multiyear average during burning seasons.”

3. Line 38, Replace “lost” by something like “average loss” otherwise it becomes slightly ambiguous (i.e. lost would correspond to the total loss over 7 years, so ~7 times 250-300 TgC/yr).

Changed “equivalent to ~37% of the carbon lost” to “equivalent to ~37% of the average carbon lost”.

4. Line 39, replace “is highest for” by either “is higher in” or “is at the highest in”

Changed to “is the highest in”.

5. Line 50, remove “dioxide”, carbon has been sequestered in different molecular forms by the vegetation.

Done.

6. Line 59, "It is in the dry season, when light becomes a key-controlling". Could be reworded. Light is a key controlling factor outside of that period as well, but it is probably less of a bottleneck.

Changed the sentence to "It is in the dry season, when more light reaches the canopy level, that the Amazon forest thrives."

7. Line 100, technically they accounted for the variability in cloudiness as those were free running simulations. However, the authors have not explicitly quantified the impact of this variability on the aerosol light fertilisation effect.

Changed the sentence to "However, the authors have not explicitly quantified the impact of Amazon background clouds and their interannual changes in tempering the aerosol diffuse radiation fertilization effect (DRFE)."

8. Line 108, The definition of CI is not clear. Does it correspond to the ratio of total (i.e. dif + dir) light at surface over the total light at ToA ? Please clarify.

The definition has been changed to "the ratio of total (i.e., direct plus diffuse) light at surface to the total incoming light at top of atmosphere".

9. Line 129, "sunlight ... drenches the trees due to reduced rain". If by "drenches" you mean "floods the canopy with light", I would use a different verb as "to drench" means "to wet thoroughly" and this is in contradiction with rest of the sentence (e. "having lesser rain").

Changed "drenches" to "shines".

10. Line 184, Replace "augmentation" with development.

Done.

11. Line 259, can probably remove "site-level" as in situ literally means on site.

Done.

12. Line 294, can replace "a regional and a time evolution" with "both a spatial and temporal view"

Done.

13. Line 296, remove "s" from observations-based

Done.

14. Line 296, put name of products in bracket after mentioning there are two.

Done.

15. Line 296, reword end of sentence for clarity g. "ecosystem productivity in the GEOS simulations"

Done.

16. Line 297, move "Through upscaling using machine learning methods (Jung et al., 2020)" to the end of the

Done.

17. Line 312-31 It's a nudged run basically isn't it?

Yes, it is.

18. Line 321, maybe I missed it in the model description (section 2), are aerosols externally mixed in GEOS-GOCART?

All aerosol components are externally mixed in GEOS-GOCART.

19. Line 330 to 344, I hope I got this correctly, in nobbaer, R2 is only used by the physiology part of Catchment-CN, is that correct? Meaning that the rest of the land surface energy budget is calculated assuming R1 as in allaer.

Yes, that's correct.

20. Fig 1b is the same as Fig 1a. State in the legend what the error bars on 1c represent.

Sorry for this oversight. The correct 1b has been inserted. The legend now includes "The error bars on 1c indicate one standard deviation of the data within each 1 km vertical layer."

21. Legend for Fig 2a and 2c specify the wavelength used for those AOD.

Done. These AODs are at 550nm.



22. Line 407, it would help the reader if the box defined here was depicted on some (all) of the contour Are all spatial averages quoted in the manuscript calculated over the same area?

Yes. We added the corresponding shaded area in Figure 2d and indicated it in line 421.

23. Line 421, maybe a reference here would be useful. Is vegetation not dark enough for MODIS dark target algorithm to perform well?

The statement here is given based on the personal study experience of our co-author. To be more rigorous, we delete this sentence.

24. Line 480 to 492. To avoid confusion, it would be better have full indexing for the radiative quantities, g.  $R_{tot@toa}$  instead of  $R_{top}$ ,  $R_{diff@srf}$  instead of  $R_{diff}$  ...

Done.

25. Line 480 to 492, As the notation has changed from the rest of the manuscript, I believe the radiation quantities here are integrated over the full SW spectrum not just PAR, is that correct? Please clarify in text.

No. All  $R_s$  are over 400-700 nm as stated in the legend of Figure 9. We now also added this information in lines 538.

26. Line 480 to 492, how were the direct and diffused components calculated here? Was any delta-rescaling of the aerosol optical properties still applied?

Thanks for the question. We have added the following information in lines 524-529: "FastjX solves the 8-stream multiple scattering in atmospheric solar radiation transfer for direct and diffuse beams, using the exact scattering phase function and optical depths of atmospheric molecules, aerosols, and clouds, and provides photolytic intensities accurate typically to better than 3%, with worst case errors of no more 10% over a wide range of atmospheric conditions (Wild et al., 2000). No special approximations are needed to treat strongly forward-peaked phase functions." Optimizations were developed for the treatment of stratospheric  $O_2$  and  $O_3$  absorption (Bian et al., 2002) to speed up the simulation.

Following Chandrasekhar, these numerical solutions generally rely on expansion of the scattering phase function in Legendre polynomials. For large aerosols and cloud droplets, however, the scattering phase function is strongly forward

peaked and not easily represented by a truncated Legendre expansion (e.g., van de Hulst, 1981). The FastjX algorithm is based on a Legendre expansion of the exact scattering phase function, and thus no adjustment to optical depth or extinction coefficient is needed. A series of numerical tests, including expansion of forward-peaked phase functions to 160 terms, demonstrates that truncation of the Legendre expansion at 8 terms gives an accurate calculation of the mean specific intensity of the radiation field. The photolytic intensity (sometimes called 'actinic flux') is the sum of the direct solar flux plus the diffuse solar flux of  $4\pi$ -steradian integrated mean specific intensity.

Since FastjX solves the 8-stream multiple scattering problem, it does not apply a delta-scaling approximation, which is typically used to improve the accuracy of the two-stream approach to correct the calculated irradiances in strongly forward scattering conditions (Joseph et al., 1976; Kylling et al., 1995).

Joseph, J. H., Wiscombe, W. J., and Weinman, J. A., 1976: The delta-Eddington approximation for radiative flux transfer, *J. Atmos. Sci.* 33, 2452–2459.

Kylling, A., Stamnes, K., and Tsay, S. C., 1995: A reliable and efficient two-stream algorithm for spherical radiative transfer: Documentation of accuracy in realistic layered media, *J. Atmos. Chem.* 21, 115–150.

van de Hulst, H. C., 1981: *Light Scattering by Small Particles*, Dover, New York, p. 471.

27. Fig 7 legend, Should the unit for GPP be kg/m<sup>2</sup>/s?

The unit of GPP has been changed to be  $\mu\text{g}/\text{m}^2/\text{s}$ .

28. Fig 7, A fifth timeseries representing the fractional / absolute change in GPP could be useful here.

Done.

29. Fig 7, Although the GPP for the site at 54W 15S is relatively high in GEOS (Fig 5C), it does not seem to be a very productive pixel in both FluxSat and FluxCom (Fig 5a and 5b) which makes sense as this is probably in the arc of deforestation. Is the tile in the model mostly covered by tall canopy PFTs or is dominated by lower grass?

Yes, this tile is located in the arc of deforestation and is covered by broadleaf deciduous temperate shrub (23%) and crop (77%).

30. Line 630, can be more specific and replace "quantities" by "GPP".

Done.

31. Fig 10 legend, BBAOD is labelled as the brown carbon part of the biomass burning aerosol, is that correct?

Corrected to "biomass burning aerosol".

32. Fig 10 legend, remove "for the ecosystem". Replace "dot-lines" by "dashed lines".

Done.

33. Fig 10 legend, replace "occurrence frequency" by "frequency of occurrence".

Done.

34. Fig 10 legend, "dGPP is 119.5% (201008) and 92.6%", hard to tell without seeing it plotted but from a quick and dirty "visual" interpolation, these numbers seems high. Can you confirm them

Yes. These numbers are high. We set a narrower range of values in plotting for easy visualization.

35. Line 628-29, sentence is already in Fig 10 legend.

The sentence has been deleted.

36. Line 635, Isn't this strong correlation to be expected from the experiment design? The atmospheric state should be pretty similar in allaer and nobbaer. The main ESM feedback allowed in nobbaer is from the vegetation that has received R2 instead of R1 isn't it? (see general comment #7 and specific comment #19). Anyway, such feedback probably has a negligible impact on cloud fields (see g. Pedruzo-Bagazgoitia et al. 2017).

The atmospheric state including cloud fields is pretty similar in allaer and nobbaer (also see the answer to comment #6). However, the background atmospheric cloud fields fluctuate naturally each year. What we examined here is whether the effect of cloud interannual change overwhelms that of aerosol diffuse radiation fertilization effect. Thanks for the reference. It has been added in line 351-352.

37. Line 632-643, In this discussion, it would be useful to quote the GPP changes (both relative and absolute) averaged over the analysis period, maybe excluding 2010 as it is an unusual year. Additionally, these period mean quantities for GPP, DFPAR, DRPAR, AOD, CLDFRC could be incorporated in a table in the main manuscript (table S1/2 a,b,c,d are useful but clearly too detailed for the main manuscript).

Done. Table 3 has been added with the information of burning season multi-year (2011-2016) averaged GPP over the Amazon region from all four simulations. Also given in the table are the GPP, DFPAR, DRPAR, CLDFRC, and BBAOD averaged during Aug-Sept, 2011-2016 over the Amazon region in all-sky condition.

38. Line 647-652, This sentence is confusing. If cloudiness is similar in 2011 and 2010, how can you conclude from this that its effects are of second order compared to aerosol effects. Surely it won't have an impact if it doesn't change.

The sentence has been removed.

39. Line 653, a followed up to comment #36. This conclusion is based on the simulation results. Other environmental controls might have affected the vegetation productivity in the real world. Could you further support your conclusion by using the observational proxies (i.e. FluxCOM and FluxSat)? One way could be to compute a GPP anomaly as GPP for 2010 minus the mean of GPPs for the other years (excluding 2010) for FluxCOM, FluxSat and the allaer simulation. Then repeat the same calculation for 2011. Seven years might be a bit short for getting a representative mean, but I believe these two datasets have records longer than the analysed period.

This is a good suggestion and a good topic for future work. Such observational anomalous GPP (e.g., FluxCom) would contain all potential impacts from environment including interannual change in temperature, cloud, precipitation, etc. These other responses could be comparable or even larger than the biomass burning aerosol diffuse radiation fertilization effect that we studied here. For example, even the climate adjustments in response to the aerosol forcing can increase the efficiency of biochemical processes (67 to 100 TgCyr<sup>-1</sup>) comparably to the stimulation of diffuse light (65 to 110 TgCyr<sup>-1</sup>) over the Amazon region (Malavelle et al., 2019).

40. Line 657 to 664, This is the part of the paper where the most novel results are introduced. This paragraph could benefit from having some further explanation on the physical meaning of the anomalies calculated here. I sort of understand

$ddX/dAOD$  as something similar to a radiative forcing (i.e. a difference in net radiative fluxes between a pair of simulations). Maybe it could be called susceptibility of the diffuse fertilization effect to BB aerosols. The double “dd” notation is a bit confusing and could be improved. It would be useful to use the same notation for the denominator (AOD) although I appreciate that the background aerosols in allaer and callaer cancel each other.

Thank you for pointing out this physical meaning. This certainly helps readers to understand our study. We have added the following sentences in lines 723-728. “This is, in principle, similar to the method of aerosol radiative forcing (RF) estimation ((i.e., estimating aerosol radiative effect (RE) with and without aerosol for present-day (pair1) and pre-industrial (pair2) and then deriving RF as a difference between the two pair REs). Here we study the sensitivity of the aerosol diffuse radiation fertilization effect to a unit change of AOD. We call it susceptibility of the diffuse fertilization effect to BB aerosols.” Also BBAOD has been changed to AOD.

41. Fig 11, Instead of (or in addition to) the mean values for 2013/15, it could be useful to have a +/- one standard deviation marks around each means at each bin points. I expect much more variability in  $ddGPP/dAOD$  at low CF than at high CF. If so, there might be a larger range of CF where the aerosol effect could be detrimental to plant growth.

Vertical bars for one standard deviation have been added. The standard deviation is indeed larger at low CF.

#### **Additional reference:**

Pedruzo-Bagazgoitia, X., Ouwensloot, H. G., Sikma, M., vanHeerwaarden, C. C., Jacobs, C. M., and Vilà-Guerau deArellano, J.: Direct and Diffuse Radiation in the Shal-low Cumulus–Vegetation System: Enhanced and Decreased Evapotranspiration Regimes, *J. Hydrometeorol.*, 18, 1731–1748, <https://doi.org/10.1175/JHM-D-16-0279.1>, 2017.