Atmospheric Chemistry and Physics Discussions

Atmos. Chem. Phys. Discuss., referee comment RC1
https://doi.org/10.5194/acp-2021-138-RC1, 2021

Comment on acp-2021-138

Anonymous Referee #1

Referee comment on "The response of the Amazon ecosystem to the photosynthetically active radiation fields: integrating impacts of biomass burning aerosol and clouds in the NASA GEOS Earth system model" by Huisheng Bian et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-138-RC1, 2021

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.

General Comments:

- 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.
- 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.
- 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.
- 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?
- 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?
- 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?
- 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?

Specific Comments:

- Line 29, change "the impact" to "this impact".

- 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.
- 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).
- Line 39, replace “is highest for” by either “is higher in” or “is at the highest in”
- Line 50, remove “dioxide”, carbon has been sequestrated in different molecular forms by the vegetation.
- 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.
- 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 variably on the aerosol light fertilisation effect.
- 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.
- 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”).
- Line 184, Replace “augmentation” with development.
- Line 259, can probably remove “site-level” as in situ literally means on site.
- Line 294, can replace “a regional and a time evolution” with “both a spatial and temporal view”
- Line 296, remove “s” from observations-based
- Line 296, put name of products in bracket after mentioning there are two.
- Line 296, reword end of sentence for clarity g. “ecosystem productivity in the GEOS simulations”
- Line 297, move “Through upscaling using machine learning methods (Jung et al., 2020)” to the end of the
- Line 312-31 It’s a nudged run basically isn’t it?
- Line 321, maybe I missed it in the model description (section 2), are aerosols externally mixed in GEOS-GOCART?
- 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.
- Fig 1b is the same as Fig 1a. State in the legend what the error bars on 1c represent.
- Legend for Fig 2a and 2c specify the wavelength used for those AOD.
- 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?
- Line 421, maybe a reference here would be useful. Is vegetation not dark enough for MODIS dark target algorithm to perform well?
- Line 480 to 492. To avoid confusion, it would be better have full indexing for the radiative quantities, g. Rtot@toa instead of Rtop, Rdiff@srf instead of Rdiff …
- 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.
- Line 480 to 492, how were the direct and diffused components calculated here? Was any delta-rescaling of the aerosol optical properties still applied?
- Fig 7 legend, Should the unit for GPP be kg/m2/s?
- Fig 7, A fifth timeseries representing the fractional / absolute change in GPP could be useful here.
- 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?

- Line 630, can be more specific and replace "quantities" by "GPP".
- Fig 10 legend, BBAOD is labelled as the brown carbon part of the biomass burning aerosol, is that correct?
- Fig 10 legend, remove "for the ecosystem". Replace "dot-lines" by "dashed lines".
- Fig 10 legend, replace "occurrence frequency" by "frequency of occurrence".
- 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?
- Line 628-29, sentence is already in Fig 10 legend.
- 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 negligeable impact on cloud fields (see g. Pedruzo-Bagazgoitia et al. 2017).
- 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).
- 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.
- 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.
- 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.
- 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.

Additional reference:

Pedruzo-Bagazgoitia, X., Ouwersloot, 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.