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Comment on acp-2021-138

Anonymous Referee #2

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-RC2, 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 Huisheng Bian et al.

General comments:

This study uses the NASA GEOS Earth System Model framework to investigate the impact of biomass burning aerosols and cloud cover on the Amazon region ecosystem productivity. This is a very interesting topic and the paper is clearly structured and generally well written.

However, while this work could certainly bring an important contribution to existing published studies on this topic, I think in its current form it still needs major revisions. I really hope this is something the authors can and will address in a revised manuscript.

Major comments:

1. A key question that needs to be addressed is whether the simulated response of GPP to changes in diffuse radiation fraction is realistic. More specifically, does the model accurately simulate observed GPP response to changes in diffuse, direct, and total surface radiation? And how does this simulated GPP response compare with other existing model estimates?

2. Why is the role of other climatic feedbacks associated with biomass burning aerosol emissions (e.g. reduction in leaf temperature) completely ignored, despite the fact that an ESM is used? While, the authors do acknowledge at the end of the paper (lines 716-719) that the aerosol induced changes in meteorological fields can also affect plant growth, this seems to be a huge missed opportunity here. Malavelle et al. (2019) showed that the overall impact of biomass burning aerosols on NPP is the net result of multiple competing effects and it would be interesting to see if similar responses are simulated with the NASA GEOS ESM system.

3. The second research objective (and the way it is addressed) is a bit unclear and it should be formulated and addressed much more clearly.

- 3a. It is evident that clouds have a substantial impact on the efficiency of the aerosol diffuse radiation effect, as they have a strong effect on diffuse radiation fraction. In a similar way it can be said that the aerosols have an impact on the efficiency of the diffuse radiation effect caused by clouds. So this in itself is not necessarily a research question.
- 3b. Is there a difference in the model between the simulated GPP response to changes in diffuse radiation fraction caused by aerosol changes and those caused by cloud cover changes?
- 3c. The fact that during the investigated period (lines 649-654), the interannual variation in regional cloudiness is small and therefore plays only a secondary role on the diffuse radiation fertilisation effect (compared to the dominant role played by the variation in biomass burning aerosol) is not surprising and does not really address the second research objective.
- 3d. Lines 657-682: The cause for the difference between the 2013 and 2015 lines in Figure 11 is suggested to be the difference in cloud cover between the two years. I wonder whether this is indeed the case, since the results illustrated in Figure 11 are in fact for binned cloud fractions anyway? I would speculate they are in fact caused by the difference in (i) biomass burning emissions (they do matter in your calculated ddX/dBBAOD, which is defined in terms of both Pair1 and Pair2) and (ii) temperature and precipitation. This needs to be investigated and clarified.

Specific comments:

- Terminology: Why is the term "aerosol light fertilizer effect" being used instead of other already established terminology, e.g. diffuse radiation fertilization effect, Mercado et al (2009). I suggest the use of existing terminology to better integrate the work with other studies, but if the authors feel strongly about introducing this new terminology, a clear rationale for this should be provided.
- Why was the this particular period (i.e. 2010-2016) chosen? Can this be extended?
- Lines 100-101 and 140-142: It is not quite true that Malavelle et al (2019) did not consider the effect of clouds altering the diffuse radiation fertilisation effect. They do in fact discuss this and mention in their paper that "despite cloudiness affecting how much aerosols can interact with radiation, we notice that NPP is enhanced in the central part of the Amazon when BBA emissions are increased (Fig. 5)." So this needs to be reformulated and clarified in this paper to avoid confusions. This points also relates to my major comment 3, i.e. the need to better define and address the second research objective.
- Lines 140-142: The authors seem to have missed other relevant studies on this topic, such as Strada and Unger (2016) and Unger et al. (2017). Results presented in this work should also be compared and integrated with those from these other studies.
- Lines 403-406: It would be good to investigate a bit more the cause of the difference in observed and simulated SSA in August at Alta Floresta. What about other periods and other sites?
- Lines 458-461: Only comparing averages over Aug-Oct 2010-2016 for simulated SW radiation and CERES measurements can potentially mask important differences. Please include an assessment and discussion of model vs measurements agreement for the full time series (e.g. 2010-2016 time series of monthly means).
- Lines 470-477: Similarly to the evaluation of SW radiation, the evaluation of simulated GPP should be investigated in more detail, i.e. time series rather than just averages.
- Figure 6: I would suggest the add another line corresponding to total radiation (i.e. the sum of the blue and red lines). This should help the discussion and better illustrate the point.
- Lines 479-516: This simulated response of total and diffuse surface radiation to different aerosol concentrations and cloud conditions needs to be evaluated against some observations. This is a key process to get right for this study and is not currently addressed in the paper. This relates to my major comment 1.
- Figure 8 and lines 580-586: An Amazon regional average GPP increase of +9.9% resulting from an increase in DFPAR of 10% is substantially larger than other existing estimates, e.g. Rap et al. (2015), Malavelle et al. (2019). However, the corresponding percentage change in NPP (lines 597-601) seems closer to estimates from other studies. It is important to investigate this further and include a discussion on why this is the case (e.g. to what extent the GPP change is driven by changes in respiration and NPP, respectively). This point also relates to my major comment 1, regarding the need to validate the simulated GPP response to changes in diffuse/total radiation against observations and/or other existing model estimates.
- Lines 605-611: The comparison with Rap et al. (2015) is incorrect and misleading. Firstly, it is incorrect because the 0.5-4.2% range of NPP change in this study is an interannual range, while the 1.4-2.8% range from Rap et al. (2015) is an uncertainty range for the 1998-2007 average due to biomass burning emissions uncertainty. The actual interannual range from Rap et al. (2015) can be inferred from their Fig. 4 and

Fig. S5. Secondly, it is misleading as the two periods are different (2010-2016 vs 1998-2007), so any comparison of interannual ranges should also include a discussion on the interannual variability in biomass burning emissions during 1998-2016.

- Lines 702-703: "The cloud fraction at which BB aerosol switches from stimulating to inhibiting plant growth occurs at ~0.8." I think this is a potentially confusing statement as it only applies to the biomass burning aerosol loadings recorded during the period investigated here. In reality, as both cloud cover and aerosol concentrations affect the diffuse radiation fraction, this threshold does also depend on the aerosol loading. A more useful threshold would be one defined in terms of diffuse radiation fraction.

Technical corrections:

- Line 32: "call here" should be "called here".
- Line 124: missing supporting citation for the 40% value.
- Line 354: typo "metrological"
- Lines 625-631: Description of figure is best included in the figure caption, with manuscript text dedicated to discussion of results.
- Lines 641-643: Please reformulate to avoid using "presumably" which is a bit too vague. A more precise statement would read much better.
- Lines 666-673: Why is a different font used in this paragraph?
- Lines 701-702: "Curiously, BB aerosols stimulate plant growth under clear-sky conditions but suppress it under full cloudiness conditions". I suggest removing the word "curiously"? This is in fact to be expected.

References:

Strada, S. and Unger, N.: Potential sensitivity of photosynthesis and isoprene emission to direct radiative effects of atmospheric aerosol pollution, Atmos. Chem. Phys., 16, 4213–4234, https://doi.org/10.5194/acp-16-4213-2016, 2016.

Unger, N., Yue, X., and Harper, K. L.: Aerosol climate change effects on land ecosystem services, Faraday Discuss., 200, 121–142, https://doi.org/10.1039/c7fd00033b, 2017.