

Interactive comment on “Potential sensitivity of photosynthesis and isoprene emission to direct radiative effects of atmospheric aerosol pollution” by S. Strada and N. Unger

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Received and published: 13 February 2016

We thank Referee #1 for their time and consideration. We closely considered the insightful and constructive comments from Referee #1. Referee #1's comments have helped us to improve the manuscript.

Referee #1's comments are quoted in italics. Authors' answer follows referee's comment in regular font.

Response to Specific Comments

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1. Additional experiments: *It would be quite interesting to see the model response to coupled changes in the aerosol emissions, e.g., 50% increase in industrial vs. 50% decrease in biomass burning, and vice versa. I think this would add another dimension to the paper, but shouldn't limit the paper's acceptance if the model simulations are too time-consuming to perform.*

Authors: Referee #1 suggests exciting ideas for future research. We do plan to explore the impacts of other changes in aerosol emissions according to possible future scenarios, i.e., increases in some sectors and simultaneous decreases in other sectors. This follow-on work will use the full land carbon cycle version of NASA ModelE2-YIBs. This paper is already rather long in its goal to examine the effects of all anthropogenic pollution emissions, biomass burning aerosols and industrial aerosols on plant productivity. Moreover, additional simulations are way beyond the scope of the present study because we have already used up the computational resources allocated for this paper.

2. Abstract: *There are couple of instances where a little more explanation is needed so that a reader can clearly understand the full details of the study. For example, on a first read I'm left wondering what “complex canopies” are, and exactly what you mean by “cooling in the Amazon Basin”. It is clear when you read the manuscript further, but not when you read the abstract alone. I suggest the authors just take a little more time to carefully improve the abstract.*

Authors: Following Referee #1's recommendations, we clarified the Abstract by: (a) replacing “complex canopies” with “forested canopies” (pag. 1, ll. 9), (b) removing the term “cooling” and by clarifying the explanation of mechanisms operating over tropical biomass regions (the Amazon Basin and central Africa) (pag. 1, ll. 15–17), and (c) stating upfront the feedbacks accounted for in the model framework (pag. 1, ll. 4–6):

“The model framework includes all known light and meteorological responses of pho-

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tosynthesis but uses fixed canopy structures and phenology.” (see Point 8 below).

3. Page 25440, lines 3: *Section number missing.*

Authors: Section number (2.1.1) has been inserted (pag. 5, ll. 152).

4. Page 25441, lines 19–23: *I had to read this twice first time around. I would bullet point these different simulation types and indicated more clearly that different emissions are removed (e.g., “all anthropogenic emissions including biomass burning are removed . . .” and “... biomass burning emissions only are removed . . .” etc.)*

Authors: To clarify the paragraph that describes the sensitivity simulation types, we follow Referee #1’s suggestion and use an itemized list where, for each simulation, the removed emissions are more clearly stated (pag. 7, ll. 209–213).

5. Section 2.2: *I can see why understand why a long simulation is needed, but a little more justification on why the first 12 years (& not 10 or 15 years) are discarded, and why only for example the last 20 years (& not 30 years) are used.*

Authors: To clarify our choices in terms of spin-up and run period, we added the following sentences: “The global atmospheric oxidant-aerosol composition usually takes about 2 years to spin-up, while the atmospheric dynamics and land-surface climate takes about 10 years to reach steady-state due to an imposed aerosol radiative forcing. Therefore, we discard the first 12 model run years as spin-up. The remaining 20 model run years are averaged for analysis. Twenty model years of data are necessary such that any aerosol-driven variable differences between the control and sensitivity simulations are statistically significant relative to internal climate model variability.” (pag. 7; ll.

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224–230).

6. Section 3.2, Page 25445, line 2: *I may be interpreting Table 1 wrong, but simNObb has a notable effect on SOA ACB reducing it from 1.37 to 1.14.*

Authors: Referee #1 is correct. Removal of biomass burning reduces SOA ACB by 17%. Hence, we completed our comments on Table 1 and highlighted the contribution of biomass burning emissions to SOA ACB by 17% (pag. 10, ll. 329–331).

7. Section 3.2.2: *I think that it would be prudent to discuss changes in leaf temperature actually here (& included figure S11 or S12 in the main manuscript) in preparation for the following discussions on isoprene emissions. Or maybe have a new section 3.2.3 instead.*

Authors: Following Referee #1’s suggestion, we modified Figure 5, which previously showed changes at the surface in atmospheric temperature (SAT) and relative humidity (RH), with a new version of Figure 5 that presents changes in transpiration efficiency (i.e., a proxy of canopy conductance), RH and canopy temperature. Relying on these new graphics, in the tropical regions we find evidence for a bio-meteorological feedback, which we discussed in detail in Sec. 3.4. By enhancing plant productivity, anthropogenic pollution aerosols increase canopy transpiration. In the north-western Amazon Basin, biomass burning aerosols drive the largest increase in transpiration efficiency, resulting in the largest decrease in canopy temperature (pag. 14, ll. 458–467). Annual changes in SAT are still commented on in the manuscript (pag. 14; ll. 442–451), and shown in Figure S3 the Supplementary Material.

8. Section 4: *I appreciate the author’s discussion on the study’s limitations. But I think not allowing leaf phenology to respond to the changes in aerosol emissions is a significant issue. Canopy inputs are changing and thus ecosystems will respond*

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accordingly. Is there anyway this effect can be quantified in this system set-up or has its likely magnitude been quantified in a similar study. I think this problem should also be directly mentioned in the abstract as well.

Authors: While we agree with Referee #1 that interactive leaf phenology and land carbon allocation may influence the vegetation response to aerosol emissions, we emphasize that this was a deliberate choice on our part for this first study that focuses on GPP and isoprene responses only. The feedbacks between vegetation and the atmospheric aerosol composition are extremely complex and challenging to disentangle. We decided it was useful and sensible to quantify the GPP and isoprene sensitivity to pollution aerosol emissions in a model framework that includes all known meteorological and light responses of photosynthesis, but that uses fixed canopy structures and phenology i.e. no feedbacks from dynamic carbon allocation or phenological timing allowed in this first study. **The goal is to provide a benchmark for future research that will add in the dynamic LAI and phenology feedbacks.** The model framework does include feedbacks from the meteorologically-altered vegetation carbon, water and energy fluxes on the atmospheric aerosol composition. We assert that the only real way that dynamic carbon allocation would potentially substantially influence the GPP response to aerosol emissions is through LAI changes, which would be a positive feedback, implying that our current results are underestimates and/or at the low end of the range of sensitivities. To our knowledge, there is only one existing published study that applies prognostic LAI (). However, that study did not isolate the effects of prognostic LAI. Our recent work with the YIBs land carbon cycle model has suggested that changes in the growing season length between 1982–2011 due to all global change drivers has little impact on regional carbon uptake and BVOC emission fluxes (). Therefore, it seems unlikely that the aerosol-induced rapid-adjustment meteorological changes described in detail in Section 3.3 would have substantial impacts on phenological timing and growing season length. Even if they did, the impacts on annual average GPP would apparently be minor. That said, our follow-on research is

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applying similar simulation methodology to assess the impacts of aerosol emissions on GPP, isoprene and other land carbon and water fluxes using a fully dynamic land carbon cycle model with interactive carbon allocation and prognostic phenology. Since the reviewer has raised important interest, we will endeavor to isolate the effects of individual global change drivers in this future research.

In the Abstract, we add the following sentences:

- “The model framework includes all known light and meteorological responses of photosynthesis but uses fixed canopy structures and phenology.” (pag. 1, ll. 4–7).
- “Future research needs to incorporate the indirect effects of aerosols and possible feedbacks from dynamic carbon allocation and phenology.” (pag. 1, ll. 19–20).

We add 2 sentences to existing explanatory paragraph in Section 2.1.1 (pag. 6, ll. 187–197):

“Linkages between vegetation and atmospheric aerosols are extremely complex. This version of the land carbon cycle model captures the meteorological (light, temperature, relative humidity, precipitation) responses of photosynthesis. The use of fixed canopy structures and phenology means that leaf mass is not driven by photosynthetic uptake of CO₂ and a closed carbon cycle is not simulated. Thus, the simulated GPP and isoprene emission are induced by light and surface temperature may alter the onset and shutdown dates of phenology.”

We add to Section 4 (pag. 18, ll. 605–606):

“Thirdly, we did not include feedbacks from dynamic LAI and phenology that may lead to an underestimation of the effects of aerosol-induced effects on plant-productivity. Future research will address these three limitations.”

9. Table 1: I would find this table quicker to understand if the changes were in per-

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centages but it is not critical. Also one could expand the table to include the relevant numbers for regional changes so as to correspond to the discussions in sections 3.3.1 to 3.3.3 of the main text.

Authors: To simplify the reading-understanding of Table 1, we provided percentage changes in ACB between the control and the sensitivity simulations. Moreover, we added two new tables (Table S1 and Table S2) to the Supplementary Material where we present changes in annual average sulfate, nitrate and SOA ACB (Table S1) and ERF (Table S2) over selected key regions.

10. Comment: *There are a lot (!) of figures in the Supplementary Material but few if any of these figures are actually referenced in the main text. If its important than they should be mentioned in the main text.*

Authors: We agree with Referee #1, hence we kept only Figures that are discussed in the main text and we reduced the number of Figures in the Supplementary Material (from 14 figures in the previous version to 8 figures in the current one).

References

- Chen, M. and Zhuang, Q.: Evaluating aerosol direct radiative effects on global terrestrial ecosystem carbon dynamics from 2003 to 2010, *Tellus B*, 66, 21808, doi:10.3402/tellusb.v66.21808, 2014.
- Yue, X., Unger, N., and Zheng, Y.: Distinguishing the drivers of trends in land carbon fluxes and plant volatile emissions over the past 3 decades, *Atmos. Chem. Phys.*, 15, 11931-11948, doi:10.5194/acp-15-11931-2015, 2015.