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Interactive Comment

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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We thank Referee #2 for their time and consideration. We closely considered the insightful and constructive comments from Referee #2. Referee #2's comments have helped us to improve the manuscript.

Referee #2 raises some pertinent points about (a) the assessment of model evaluation, (b) the complexity of Section 3.2 and (c) the unique attribution of changes in land carbon fluxes to prevailing mechanisms. Following Referee #1's recommendation, we completed model evaluation with computation of the linear correlation Pearson's coefficient (Pearson's R), the Pearson's R squared (R^2) and the root-mean-squared error

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($RMSE$). In the revised manuscript, we re-organized Section 3.2 to discuss separately regional changes in aerosol pollution (now Sec. 3.2), surface solar radiation (now Sec. 3.3) and surface meteorology (now Sec. 3.4), and we inserted Table 5 that gathers changes in annual averages over target regions. Together with an attempt to clarify the discussion of results, we indicate the main limits of our approach, including the opportunities and challenges to linking aerosol-driven changes in surface meteorology to the changes in land carbon fluxes.

Referee #2's major comments are quoted in italics. Authors' answer follows referee's comment.

Response to General Comments

1. Section 3.2.1*: I found Sections 3.2.1 a bit difficult to follow. May I suggest treating the results for direct and diffuse radiation each alone in their own subsections/paragraphs, followed by a summary on the net impact on total radiation? Or alternatively, treat each region in their own separate subsections/paragraphs? In my opinion, this section could be refocused so that it's more consistent with what is important for understanding the results presented in Section 3.3.1 and 3.3.2. I was not convinced how the effects of cooling and scattering were unequivocally separated later in the manuscript, and I suspect that could be laid out more clearly in the presentation of results here. I also had trouble being convinced of some of the regional comparisons that were being made.*

Authors: We agree with Referee #2 that the structure of Section 3.2 needed to be improved and clarified. For this reason, we re-structured the whole Section 3 and chose to discuss, in the order, contributions of aerosol pollution from the different sensitivity simulations (Sec. 3.2), aerosol-driven changes in surface solar radiation (Sec. 3.3) and in surface meteorology (Sec. 3.4), and, at last, changes in land carbon fluxes (plant productivity in Sec. 3.5, and isoprene emissions in Sec. 3.6). In each section, we firstly present changes at the global-scale, afterwards we focus on changes in five key regions: eastern North America, Eurasia, north-eastern China, the north-western Amazon Basin and central Africa. We agree with Referee #2 that comparisons among key regions were not clear from the figures showing the spatial distribution of annual/seasonal changes. Hence, we added a new table in the revised manuscript (Table 5) that presents, for each of the five key regions, absolute and percentage changes in annual average surface radiation, canopy temperature, GPP and isoprene emissions. For the five key regions, absolute and percentage changes in seasonal averages are reported in the Supplementary Material (Table S3 and Table S4). In the revised manuscript, the comparison between key regions is now mostly based on results gathered in Table 5. The methodology to compute absolute and percentage differences in annual and seasonal averages over selected key regions is presented in Section 2.2 (pag. 7–8, ll. 231–237).

2. p. 25446, lines 5–6: *It's unclear from Figure 4 that the eastern US shows much larger of an increase in diffuse radiation than over China for example (especially looking at panel (i)). This point seems important further on in the article, so I think it deserves further clarification.*

Authors: Table 5 should now help in clarifying this point. Table 5 shows a larger increase in annual average diffuse radiation over eastern North-America compared to Eurasia and north-eastern China due to all anthropogenic aerosols (pag. 13, ll. 415–420):

"The eastern North America shows the largest increase in annual diffuse radiation due to all anthropogenic aerosols ($+8.6$ W m^{-2}; $+6.2$ %), followed by north-eastern China and central Africa, which experience similar changes ($\sim +7.4$–7.9 W m^{-2}; $\sim +5.7$ %). Over the eastern North-America, the increase in diffuse radiation maximizes during boreal summer ($+13.6$ W m^{-2}; $+8.9$ %), with changes that are 1.6–5.7 W m^{-2} (1.9–

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$3.3\,\%$) higher that those observed over north-eastern China and Eurasia (Table S3 in the Supplementary Material)."

However, in response to non-biomass burning aerosols, eastern North-America, Eurasia and north-eastern China show similar increases in diffuse radiation (pag. 17, ll. 568–570):

"In response to aerosol pollution from non-biomass burning sources Europe and China show a large decrease in annual average direct radiation (-24–26%), but a similar increase in diffuse radiation ($+3$–$5\,\%$) as eastern North America (Table 5).".

3. p. 25446, lines 8–11: *The authors state that biomass burning aerosol drive the decrease in several regions (in the range of -6 to -28 W m^{-2}), but as I look at Figure 4 over the regions named, it seems to me that subtracting the industrial sources also result in decreases on the order of -6 to -12 W m^{-2} and larger. This seems especially true when looking at the seasonal results in Figure S6. Am I misinterpreting the plots?*

Authors: Referee #2 is correct. In the revised manuscript, the insertion of Table 5 should make this point clearer and illuminate the comparison of impacts of biomass burning and industrial sources on surface radiation. In industrialized key regions (i.e., eastern North America, Eurasia and north-eastern China), industrial aerosols (non-biomass burning aerosols) mostly drive changes in surface radiation (pag. 13, ll. 415–425). On the contrary, in biomass burning key regions (i.e., the north-western Amazon Basin and central Africa), biomass and non-biomass burning aerosols share a similar contribution to changes in surface radiation (pag. 13, ll. 425–429).

4. Section 3.3.1 and 3.2.1: *To pick up on this a little more, I also had some trouble with Section 3.3.1. Many of the conclusions here seemed to depend on contrasting the magnitude of certain effects over various regions. However, when I would try to corroborate the statements by consulting the Figures myself, in some cases the magnitudes*

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didn't appear to be all that different. This might have to do with the Figures themselves, or maybe this could be improved by refocusing Section 3.2.1. In some cases, perhaps (re-?) stating some of the actual values would help.

Authors: We agree with Referee #2 that figures showing the spatial distribution of annual/seasonal changes between the control and the sensitivity simulations do not provide a proper support to compare the magnitude of aerosol-driven impacts in the five key regions. To answer to this point and underpin discussion of results, in the revised manuscript we added Table 5 (plus Table S3 and Table S4 in the Supplementary Material) to show changes (absolute and percentage changes) in annual (seasonal in the Supplementary Material) average surface radiation, canopy temperature, GPP and isoprene emissions. In the revised manuscript, the quantitative discussion of results is now mostly based on values summarized in these tables (i.e., Table 5 in the main text, Table S3 and Table S4 in the Supplementary Material).

5. p. 25450, lines 4–8: *I don't see from Figure 4 how the increase in diffuse radiation over the eastern US is that much larger than over China and parts of Europe (as I mentioned above). Moreover, it's not at all convincing from Figure 5 that SAT over the eastern US is "reduced". There is a very small isolated patch of blue, but there is no hatching anywhere to denote significance, and most of the region is blank. I'm also confused as to what is "contrary" about Europe and China experiencing a strong reduction in total and direct radiation. Panel 4a and 4b show the US experiences comparable decreases in total and direct radiation as for parts of Europe, and maybe China. Maybe part of this confusion can be clarified by better summary of the results of Figure 4 in Section 3.2.1?*

Authors: We attempt to avoid confusion via Table 5, plus Table S3 and Table S4 in the Supplementary Material. As described in point 2, anthropogenic pollution aerosols drive a larger increase in annual average diffuse radiation over eastern North-America

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compared to Eurasia and north-eastern China (pag. 13, ll. 415–420). However, non-biomass burning aerosols (non-BBAs) drive similar increase in diffuse radiation in eastern North-America, Eurasia and north-eastern China (pag. 17, ll. 568–570).

In terms of total and direct radiation, due to non-BBAs, Eurasia and north-eastern China undergo the largest reduction in total and direct radiation. Over Eurasia and north-eastern China, decreases in total and direct radiation maximize during boreal summer, with changes that double those observed over eastern North-America (pag. 13, ll. 423–425).

6. p. 25450, lines 22–23: *I can see from Figure 4 how it might be true that the increase in diffuse radiation over the Amazon is weaker than over central Africa – but it doesn't seem that different, either. As a matter of fact, Section 3.2.1 places the two regions in the same sentence within the same range . . . So it's not clear how the statement "the Amazon basin experiences a weaker increase in diffuse radiation" can be all that significant. Again, this might be helped by better structuring Section 3.2.1 to correspond to the conclusions being made here in Section 3.3.1 (and/or by referring to exact values over specific regions, for diffuse and direct radiation separately). Likewise, the "larger cooling" experienced by the Amazon basin compared to central Africa (Figure 5) doesn't appear notable to me either. In Panel 5a, they have roughly the same amount of area that is hatched as significant. This statement seems important to their conclusions about how "cooling dominates in the Amazon basin", but as is, I think the authors need to do a better job showing that this is true.*

Authors: In the revised manuscript, we reformulate our hypothesis regarding aerosol-driven effects on tropical regions (Sec. 3.4, pag. 13–15). In the model, photosynthesis and stomatal conductance are coupled through the Farquhar-Ball-Berry approach. Direct radiative forcing (DRF)-driven increases in photosynthesis and GPP are associated with increases in canopy conductance and relative humidity via increased transpi-

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ration. Due to BBAs, the north-western Amazon Basin records the largest increase in transpiration efficiency and, as a corollary, the largest decrease in canopy temperature (-0.31 K; -0.10 %), which is ~ 0.1 K larger than the decrease in canopy temperature over central Africa and north-eastern China. (pag. 13, ll. 460–467). We name this as "bio-meteorological effect" since reductions in the canopy temperature observed in the north-western Amazon Basin represents a positive feedback on plant productivity (further increases) in response to the DRF-driven increases. The same bio-meteorological effect (i.e., robust decrease in canopy temperature and corresponding GPP enhancement) seems to operate also in central Africa and north-eastern China; these regions undergo additional substantial robust reductions in direct radiation. In central Africa, the analysis of seasonal changes in GPP reveals that enhancement in GPP maximizes in boreal autumn, together with decrease in canopy temperature, while reductions in direct radiation maximizes in boreal summer (pag. 16, ll. 524–529).

7. p. 25451, lines 12–16*: Again, given the results that have been presented, I'm not yet convinced that the different mechanisms for each region (light scattering over Eastern US; reductions in direct radiation in Europe and China; cooling in the Amazon Basin) could have been established from the present model results alone. In my opinion, the arguments leading up to this based on the present model results alone have not been clearly developed.*

Authors: Please see Responses to points (1)–(6) above. We agree with Referee #2 that the way our original ideas were presented may not have been conclusively supported by the simulations results available to us. The new Table 5 makes the key drivers and processes across regions more quantitatively apparent and transparent. Of course, more than one mechanism operates in each region, and a confounding issue is that the mechanisms are not independent of each other. Therefore, given the quantitative data available to us from the completed global simulations e.g. as presented in Table 5, we identify the predominant mechanism in each region while fully

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recognizing the complexity of aerosol-meteorology-vegetation interactions.

As a final note, we expect the analyses to become increasingly complex when we turn on the dynamic carbon allocation and prognostic phenology. Therefore, in our on-going project work, we are developing a standalone version of YIBs that includes a fully coupled atmospheric radiative transfer scheme, which will be applied in our future studies.

Response to Specific Comments

1. Section 2.1 p. 25441, line 2*: Is there a particular reason that the Unger et al. 2013 ACPD article is being cited, when the ACP article is available?*

Authors: For Unger et al. (2013), the correct reference, which concerns the ACP article and not the ACPD, has been entered in the Reference list.

2. Section 2.2 p. 25442, line 16*: Can you state/show some of the IPCC values that you are referring to for comparison, so the reader can see how consistent the results here are?*

Authors: Following Referee's #2 comment, we compare NASA ModelE2-YIBs to ERF and RF values from the IPCC AR5 values in Section 3.1 (pag. 8, ll. 241–255). Although the ERF and RF concepts differ, since our study only encompasses the direct aerosol effect and since the IPCC AR5 report only presents RF by single component, we compare IPCC AR5 RF values to the corresponding ERF simulated by NASA ModelE2-YIBs, which are reported in Table 2 (i.e., SimCTRL−SimNOant values).

3. p. 25442, line 23*: This is certainly on the low end of the global isoprene emissions*

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estimate. Could you comment on why this might be?

Authors: We inserted comments about the global isoprene estimation provided by NASA ModelE2-YIBs and changed the sentence to (pag. 8, ll. 260–266):

“The global isoprene source is 402.8 TgC yr^{-1} , which is at the low end of the range of previous global estimates (e.g., 400–700 TgC yr^{-1}, Guenther et al., 2006). However, a recent study suggests a larger range of 250–600 TgC yr^{-1} (Messina et al., 2015). The photosynthesis-based isoprene emission models tend to estimate a lower global isoprene source than empirical models because the scheme intrinsically accounts for the effects of plant water availability that reduce isoprene emission rates (Unger et al., 2013).”.

***4. Sections 3.1.1 and 3.1.2**: It’s not clear to the reader how “consistent” the AOD and GPP results are with observations. While the Figures do a good job showing that the model can broadly reproduce some of the spatial patterns, could some quantifiable statistics from the comparisons be shared?*

Authors: As suggested by Referee #2, we add a new table (Table 4) to present quantifiable statistics regarding model evaluation against observations: MODIS for coarse aerosol optical depth (AOD), and global FLUXNET-derived gross primary productivity. In the revised manuscript, for annual and seasonal (i.e., boreal summer and winter) average, Table 4 reports: the linear correlation Pearson’s coefficient (Pearson’s R), the Pearson’s R squared (R^2) and the root-mean-squared error ($RMSE$). Comments concerning model evaluation and Table 4 are reported in Section 3.1 (for coarse-AOD: pag. 9–10, ll. 301–307; for GPP: pag. 10, ll. 316–319). Moreover, for GPP, we reported the main results of site-level evaluation of the model YIBs as performed in () (pag. 10, ll.

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320–323).

5. Section 3.2.1: p. 25445, lines 14–16*: Should the authors clarify when they say "slightly affected" or "highly sensitive" that they are referring to the relative change (%)? The absolute magnitudes seem roughly equally considerable (~ 2–8 W m^{-2}).*

Authors: We rephrased the mentioned sentence and referred to changes, precising the range of absolute and percentage changes in parentheses (pag. 12, ll. 392–397):

"Relative to the control simulation (SimCTRL), changes in global total and diffuse radiation are slightly affected by the pollution aerosol burden (absolute change for total radiation: from $+1.6$ W m^{-2} to $+5.1$ W m^{-2}; absolute change for diffuse radiation: from -1.3 W m^{-2} to -3.8 W m^{-2}; relative change: 1.7–$2.5\,\%$). On the contrary, changes in direct radiation shows a larger sensitivity range to the aerosol burden (absolute change: 2.9–8.9 W m^{-2}; relative change: 3.6–$11.2\,\%$)."

Following Referee's #1 suggestion, in Table 3 we provided relative changes between the control and the sensitivity simulations to help the readers interpreting results.

6. p. 25445, line 26*: I think a word ("atmosphere"?) is missing between "aerosol laden" and "due to".*

Authors: Since we modified the whole structure of Section 3.2.1 (now Section 3.3), the designated sentence is no more included in the revised manuscript.

7. p. 25445, lines 25–27*: These lines seem to essentially repeat statements from the immediately preceding paragraph (lines $\sim 12--14$). Perhaps make it clearer that while the Table is global totals, Figure 4 shows the spatial distribution of the impacts.*

Authors: In the revised manuscript, we modified the whole structure of Section 3.2.1

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(now Section 3.3), and we decided to comment, first, aerosol-driven changes in surface radiation at the global-scale via global totals gathered in Table 3 (Sec. 3.3.1). Afterwards, we present aerosol-driven changes in surface radiation in the five key regions by briefly commenting on Figure 4 and mainly comparing regions based on Table 5, which summarizes changes in annual average in the five key regions.

To clarify the content of Table 3, we inserted the following sentences at the beginning of Section 3.3.1 (pag. 12, 390–391):

“The global annual average shortwave visible solar radiation (total, direct and diffuse) for each simulations (control and sensitivity) are gathered in Table 3.”.

To clarify the content of Figure 4, we inserted the following sentences at the beginning of Section 3.3.2 (pag. 13, ll. 409–411):

“Figure 4 shows the spatial distribution of aerosol-driven annual absolute changes in surface radiation (for annual percentage and seasonal absolute changes: Fig. S1 and S2 in the Supplementary Material).”

8. p. 25446, line 17*: Correct “of” to “or”.*

Authors: Since we substantially modified Section 3.2.1 (now Section 3.3), the designated and uncorrect sentence (“responsible of” instead of “responsible for”) is no more included in the revised manuscript.

9. Section 3.2.2 p. 25448, lines 14–15*: An explanation for how the changes will be linked to SSR and SAT uniquely might be useful here.*

Authors: Following Referee #2’s suggestion, in the opening of Section 3.4 we remind that our experiments use fixed SSTs and do not consider aerosol indirect effects in clouds. These methodology limits the influence of pollution aerosols on the Earth System to direct changes in surface radiation that affect the atmosphere and land-surface only. For this reason, we mainly relate changes in land carbon fluxes to changes in surface radiation, surface meteorology (e.g., SAT) and plant conditions (e.g., transpiration, canopy temperature) (pag. 13, ll. 437–441).

In Section 3.4, we highlight as well that, by allowing rapid adjustments for the atmosphere and land-surface only, we do not observe significant changes (at 95 % confidence level) in SAT (on a global scale), nor in precipitation rate or cloud water content due to anthropogenic aerosol pollution (pag. 14, ll. 442–446).

10. Section 3.3.1 p. 25449, lines 24–27: *The authors comment on how the impact is greatest for PFTs with complex canopy architectures. Maybe the evidence of this is found in the Figure, but this it's not explained clearly. Please elaborate.*

Authors: As suggested by Referee #2, we better explained how we assert that larger impact on GPP are observed in complex-forested canopy architectures with high tree heights and multiple layers (pag. 16, ll. 530–538).

11. Section 3.3.3 p. 25453, line 19: *"not sensitive" – Can you clarify how you've decided this? Do you mean that within 95% CI, there is no significant change?*

Authors: In the revised manuscript, the referred sentence has changed. Thank to comments of Referee #2, we corrected discussion of annual changes in isoprene emission in the north-western Amazon Basin, and we related changes in isoprene emission to changes in GPP. Actually, in the north-western Amazon Basin biomass burning aerosols drive a statistically significant (at $95\,\%$ confidence level) rise in isoprene emissions ($+0.4$ TgC yr^{-1}; $+2.4\,\%$), although the area of statistical significance is small (pag. 17–18, ll. 581–584):

"In the north-western Amazon Basin, annual average isoprene emission increases are

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simulated in response to BBAs ($+0.4$ TgC yr^{-1}; $+2.4\,\%$) (Table 5), although the area of statistical significance is small. In this region, the influence of increases in GPP on isoprene emission over-rides the influence of the cooler canopy temperatures (Table 5)."

Taking into account Referee #2's comments, at the end of Section 2 ("Methodology"), we defined the use of the adjective "significant" to refer to absolute/percentage changes that are statistically significant at $95\,\%$ confidence level (pag. 7–8, ll. 235–267).

12. p. 25454, lines 2: *Insert a period between "US" and "This region . . . "*

Authors: Since we substantially modified Section 3, the designated sentences are no more included in the revised manuscript.

13. Section 4 p. 25454, line 23–24: *I think the authors could include a brief comment about how aerosol pollution can drive plant phenology.*

Authors: Following Referee #2's suggestion, we commented about aerosol-driven changes in surface radiation and temperature that may affect plant phenology (pag. 6, ll. 194–197):

"For example, aerosol-induced effects on light and surface temperature may alter (i) the onset and shutdown dates of photosynthesis and growing season length (Yue et al., 2015a) (ii) the carbon allocation, LAI and tree height that provide a feedback to GPP (Yue et al., 2015b)."

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References

Yue, X. and Unger, N.: *The Yale Interactive terrestrial Biosphere model version 1.0: description, evaluation and implementation into NASA GISS ModelE2*, Geosci. Model Dev., 8, 2399–2417, doi:10.5194/gmd-8-2399-2015, 2015.

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