

Response to Referee #2

We are grateful to the reviewer for their time and energy in providing helpful comments and guidance that have improved the manuscript. In this document, we describe how we have addressed the reviewer's comments. Referee comments are shown in black italics and author responses are shown in blue regular text.

1 Overall assessment and general comments

In this manuscript, focusing on China, the authors apply a process-based vegetation model and a column radiative model (CRM) to regionally assess the effects of present-day aerosol loading on Net Primary Productivity (NPP). By performing sensitivity studies under different aerosol optical depth (AOD), the authors estimate two AOD thresholds that (1) leads to maximum NPP (AODt1) and (2) always enhances local NPP (AODt2). This original estimate provides a tool to evaluate the possible impact an increase/decrease in the regional aerosol loading may have on the land carbon uptake. In their assessment, the authors account as well for the role of clouds, compared to aerosols, in the diffuse fertilization effect (DFE) by analyzing both clear-sky and all-sky conditions in the model output.

The paper examines an important topic such as the aerosol DFE, and addresses relevant scientific questions over a region critical for air pollution studies. Hence, the paper is within the scope of ACP. The abstract is concise and complete, the paper is well written, the methods and modeling are well laid out, the literature is thoroughly referenced, and the results are presented in good clear figures, with an appropriate use of supplementary materials. Overall, I recommend publication after a few minor comments, listed below, have been answered by the authors. In particular, I would suggest the authors to make the title more precise, and to better outline the originality of the developed method (i.e., AOD thresholds) and how this may provide a useful tool for better understanding the role of aerosols in the DFE.

→ We agree that the initial title was not precise and did not reflect the novelty of the study. We have changed the title to “Aerosol optical depth thresholds as a tool to assess diffuse radiation fertilization of the land carbon uptake in China”. The new title captures the two main novel contributions of this study (AOD thresholds and aerosol radiative effects on NPP in China), and their connection.

2 Specific comments

Sect. 1, Introduction

Introduction is exhaustive, clear and has a right length. I found Table 1 a very good state-of-the-art on observational studies of cloud and aerosol DFE. Regarding Table 1, may I suggest the authors to account for the following observational studies that focus on cloud DFE?

- “Variations in the influence of diffuse light on gross primary productivity in temperate ecosystems”, Cheng et al., *Agricultural and Forest Meteorology*, 2015.
- “Using satellite-derived optical thickness to assess the influence of clouds on terrestrial

carbon uptake”, Cheng et al., Journal of Geophysical Research, 2016.

→ Yes, the two papers well match the scope of this study and have been added to Table 1 for the completeness of literature review.

Pag. 4, ll. 91: “Our model approach offers a large regional scale assessment ...” It’s not clear to me the reason why the Mercado et al.’s study is cited at the end of the this sentence. Could you please clarify this sentence to me?

→ The citation to Mercado et al. (2009) is incorrect at the end of this sentence. We have removed it in the revised manuscript.

Sect. 2, Methods

Methods are clearly outlined. To allow the traceability of results, I think it is important to provide details on the time and spatial scale of used dataset (e.g., FLUXNET and MODIS) and as well the MODIS products that have been used (e.g., MODIS Terra and/or Aqua? Original time and spatial scale of MODIS product? Which MODIS product?).

→ We explained the temporal coverage of GPP, NPP, and AOD data as follows:
“... we use global benchmark product upscaled from the FLUXNET eddy covariance data for 2009-2011” (Lines 239-240)
“For NPP, we use the satellite product of 2009-2011 retrieved by the Moderate Resolution Imaging Spectroradiometer (MODIS)” (Lines 240-242)
“We also use satellite-based AOD data of MOD08_M3 for 2008-2012 retrieved by MODIS onboard the Terra platform” (Lines 259-261)

We explained the spatial information of these data as follows:
“All gridded data are interpolated onto $1^{\circ} \times 1^{\circ}$ grids, matching the resolution of both CRM and YIBs models.” (Lines 261-262)

Pag. 8, ll. 217: “The simulated PAR is alternately applied ...” It’s not clear to me the use of the adverb “alternately” in this sentence. Could you please clarify this sentence to me?

→ Simulations with YIBs are performed for 2000-2011, which is longer than the period of 2009-2011 for PAR simulations with CRM. As a result, we have to recycle the PAR data as input. We added the following sentence to clarify: “In this case, predicted PAR at 2009-2011 is recycled as input for periods of 2000-2002, 2003-2005, 2006-2008, and 2009-2011 in the YIBs simulations.” (Lines 232-234)

Sect. 3, Results

Pag. 10, ll. 287–289: The authors state that “Introduction of aerosol pollution to this system . . . thus increasing the LUE and GPP of the whole canopy.”. I’m not sure if this

sentence refers to the behaviour of GPP at DF lower or greater than 0.55. As I look at Fig. 4, if I understand correctly, the DF enhances under an increasing aerosol loading. For clear-sky conditions (red empty points) at diffuse fraction (DF) < 0.55, I can clearly see that GPP enhances as DF increases. However, at DF > 0.55, it's not easy to understand the effects a further introduction of aerosols has on GPP. Could the authors make this point clearer to me?

→ Yes, the DF enhances under an increasing aerosol loading. We clarified it at the beginning of the paragraph: “Appearance of cloud and/or aerosols increases DF but decreases total insolation.” (Line 305)

The sentence refers to low DF (DF < 0.55) conditions, we clarified as follows: “At low DF (DF < 0.55), shaded leaves experience low light availability because diffuse radiation is limited. Meanwhile, photosynthesis of sunlit leaves is light-saturated because direct radiation is abundant. Introduction of aerosol pollution, which increases DF, to this system redistributes sunlight to the shaded light-limited leaves (without compromising the total light availability to sunlit leaves), thus increasing the LUE and GPP of the whole canopy.” (Lines 308-312)

We explained why high DF (DF > 0.55) will decrease GPP as follows: “However, at high DF (DF > 0.55), light is no longer saturated for sunlit leaves because of the large attenuation of direct light. Further introduction of aerosols decreases photosynthesis of sunlit leaves, which may offset the carbon gains from enhanced diffuse light by shaded leaves, resulting in a net carbon loss for the whole canopy.” (Lines 316-320)

Pag. 10, ll. 291–292: “...for shrub, ...for C3 herbs, and ...for C4 herbs”. It's not clear to me if “shrub” includes as well the tundra PFT (as seems to be stated further in the text, pag.11, ll. 311), or if this PFT has been discarded for analysis. May I suggest to specify as well PFTs included under “C3/C4 herbs”?

→ We have specified PFTs for shrub and C3/C4 herbs as follows: “... 5.6 ± 1.7 for shrub (tundra plus arid shrub), 2.8 ± 0.7 for C3 herbs (C3 grassland and cropland), and 2.2 ± 2.3 for C4 herbs (C4 grassland and cropland) ...” (Lines 314-316).

Pag. 12, ll. 363–367: “Over the North China Plain and the Southwest, ... relative to aerosol-free conditions.” Many of the results discussed here seemed to refer to contrasting magnitudes over selected regions. However, when I consult Fig. 7 by myself, trying to corroborate the statements, in some cases I couldn't find the same conclusions. For example: over the North China Plain, current AOD levels seem to me lower than AODt2 during summer. I also have trouble in validating conclusions over southeastern coastal regions, where it seems to me that observed AOD is lower than both AODt1 and AODt2 (and not “lower than AODt2 but close to AODt1”). Am I misinterpreting the plots (Fig.7b and d)? Maybe, stating some of the actual values could help the reader in consulting these plots.

→ For North China Plain, our former explanations are incorrect and have been revised as follows:

“In North China Plain, AOD exceeds AOD_{t1} and is close to AOD_{t2}, indicating that aerosol pollution there has almost neutralized impacts on local NPP.” (Lines 395-397)

For southeastern regions, our former explanations are not clear. We clarified as follows:

“For the Southeast, current AOD is lower than AOD_{t1} and AOD_{t2} at confined regions along the coast, but is higher than AOD_{t1} in the inner domain. On average, observed AOD of the box domain e (Fig. 6a) is lower than AOD_{t2} but close to AOD_{t1} (Fig. 6e)” (Lines 397-399)

We did not present digital values here because most of these quantified results have been shown in Fig. 6 and discussed in the previous paragraph.

Pag. 13, ll. 372–374: Concerning Fig. 8, I found interesting that under both clear-sky and all-sky conditions, changes in summer NPP are very small ($\sim 0\text{gC m}^{-2} \text{day}^{-1}$) over the North China Plain, although this region shows the highest levels of summer AOD (Fig. 3). Is it possible to provide an explanation of results in Fig. 8 based on Fig. 7? In my opinion, results presented in Fig.8 should be better contextualized in the whole study.

→ The reason why changes in NPP are very small in North China Plain at both clear-sky and all-sky conditions is that background (aerosol-free) NPP is low there. We plotted fractional changes (in percent) of NPP in a new Figure S7 and clarified as follows:

“The absolute changes in NPP are small over North China Plain (Fig. 8a), where high AOD is observed (Fig. 3). In contrast, fractional percentage changes in NPP exhibit a high center of >60% in North China Plain, consistent with the conclusion of sensitivity tests that aerosols usually promote carbon uptake at clear sky (Fig. 5). Such discrepancy originates from the low background (aerosol-free) NPP in North China Plain, because the YIBs model applies satellite-based land cover (Fig. S1), which shows high fraction of C3 cropland but almost zero tree (including ENF and DBF) coverage in North China Plain.” (Lines 408-415)

We provide more detailed explanation of NPP changes in Figure 8 based on results in Figure 7:

“The spatial pattern of percentage NPP changes (Fig. S7b) highly resembles the AOD differences shown in Fig. 7d but with opposite signs. Over the Southwest and part of North China Plain, current high level of AOD exceeds AOD_{t2} and as a result inhibits local NPP by 1-2%. In the southeastern coastal regions, aerosol DFE is limited, though regional AOD is below AOD_{t2}. The largest NPP enhancement is predicted over the Northeast, where current AOD is far smaller than AOD_{t1} (Fig. 7c) and cloud amount is moderate (Fig. S4).” (Lines 418-424)

3 **Minor comments**

Abstract - Pag. 2, ll. 23: Definition of the acronym DFE is missing in the abstract (latter defined in the main text, pag. 3, ll. 47). Please insert a definition in the abstract.

→ We defined DFE in abstract as “diffuse fertilization effect (DFE)” (Line 50)

Sect. 1, Introduction Pag. 3, ll. 46: To establish common ground with readers, may I suggest to add a short definition of LUE (e.g., GPP/PAR)?

→ We added LUE as “(LUE = GPP/PAR, GPP is gross primary productivity and PAR is photosynthetically active radiation)” (Lines 48-49)

Pag. 3, ll. 72: “and the plant species” Again, to establish common ground with readers, I think it would be useful to briefly precise some plant features that influence the DFE.

→ We added explanations about how plant species affect DFE in the sentences before: “Photosynthetic response to diffuse light is also dependent on plant functional type (PFT). C4 plants are less sensitive to the enhanced diffuse radiation compared to C3 plants because C4 plants do not become light saturated under high irradiance. As a result C4 plants are more sensitive to the reductions in direct light than C3 plants” (Lines 73-77)

Pag. 4, ll. 76: “Observations suggest that both cloud and aerosols exert . . .”, I think an “s” is missing in “cloud”.

→ Added “s” after “cloud” as suggested (Line 83).

Sect. 2, Methods

Pag. 5, ll. 117: Definition of the acronym PFT is missing. Please define it.

→ We added the definition at the first place it appears: “plant functional type (PFT)” (Line 74)

Pag. 5, ll. 136: May I suggest to specify here that the CRM model needs aerosol profiles and meteorological re-analyses to calculate “reflectivity and transmission of atmospheric layers . . .”? As already done by the authors, the applied aerosol profiles and meteorological re-analyses will be specified later.

→ We added following sentence to clarify: “Using temporal-varying aerosol profiles (types and concentrations) and meteorological reanalyses, the CRM model calculates reflectivity and transmission of atmospheric layers ...” (Lines 147-149)

Pag. 6, ll. 149: “The model utilizes ...” may I suggest to precise “the CRM model”?

→ Corrected as suggested (Line 161).

Pag. 8, ll. 230: “We then select sites that all months are available ...”, I think “that” should be replaced with “where”.

→ Corrected as suggested (Line 252).

Sect. 3, Results Pag. 10, ll. 277: “. . . because lower cloud coverage there allow larger ...” I think an “s” is missing in “allow”.

→ Corrected as suggested (Line 299).

Pag. 11, ll. 328: “...both cloud and aerosols exert ...” I think an “s” is missing for “cloud”.

→ Corrected as suggested (Line 355).

Sect. 4, Discussion and conclusions Pag. 13, ll. 387–388: “. . . available measurement and modeling results ...”: I think an “s” is missing in “measurement”.

→ Corrected as suggested (Line 436).

Pag. 13, ll. 390: “. . . radiative transfer scheme, We apply . . . ” Replace comma with dot.

→ Corrected as suggested (Line 438).

Sect. 2, Methods Figures 1–3: For completeness, I would suggest to insert a short explanation of what red and dashed lines represent.

→ In the captions of Figures 1-3, we added an explanation for red and dashed lines: “The dashed line represents the 1:1 ratio. The red line is the linear regression between simulations (predictand) and observations (predictor)”.