Atmospheric Chemistry and Physics Discussions

Atmos. Chem. Phys. Discuss., referee comment RC1
https://doi.org/10.5194/acp-2021-701-RC1, 2021

Comment on acp-2021-701

Anonymous Referee #1

Referee comment on "Distinguishing the impacts of natural and anthropogenic aerosols on global gross primary productivity through diffuse fertilization effect" by Hao Zhou et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-701-RC1, 2021

"Distinguishing the impacts of natural and anthropogenic aerosols on global gross primary productivity through diffuse fertilization effect", by Hao Zhou et al.

The manuscript by Hao Zhou and colleagues assesses the impacts of present-day aerosol loading on the scattering of shortwave radiation and its impacts on gross primary production in terrestrial ecosystems. It does so by using a suite of models: GEOS-Chem to simulate aerosol concentrations from multiple sources, CRM to compute the impacts of these aerosols on direct and diffuse shortwave radiation, and YIBs to simulate the impacts of direct/diffuse radiation on primary production.

Overall, this is an interesting study that attempts to break down the global-scale diffuse fertilization effect into contributions from different aerosol types and distinctions between anthropogenic and natural impacts. The topic is suited for publication in ACP, and assessments like the one presented here are helpful to shed light on the importance of the various aerosol-related impacts on radiation and productivity, and on the most important drivers of these impacts. However, the description of the setup in the current manuscript is often presented in too concise a manner, which makes it hard to assess exactly how the impacts were computed and which other factors may play a role. Also, I have some questions about the setup that the authors might have thought of/addressed already in their setup, but that are not explained in the text. I list these major shortcomings below, followed by a list of minor comments that the authors could address when revising the manuscript.

Firstly, the study consequently expresses impacts of aerosols on PAR and GPP as increase or decrease, but it is not mentioned what the reference is for this. I trust that these are all expressed relative to the simulations without any aerosols, but I urge the authors to clarify this, and would recommend avoiding using "increase" or "decrease", because these imply a trend in time and not an effect relative to a (hypothetical) reference case.

Also, the study makes a number of simplifications that are not spelled out in the text, but that I think should be explicitly mentioned and discussed. Most importantly, in the presentation of the results, the study treats the effects of different aerosols (anthropogenic/natural, BC/OC/Sulfate+Nitrate/Sea salt/dust) to be additive and independent, resulting in contributions to the radiation effects and GPP that nicely add up to 100% (l. 189ff, l. 316ff, Fig. 3, Fig. 4). However, because many non-linearities exist in the radiation responses to aerosols and the GPP responses to radiation, the sum of the individual effects will not be similar to the total effect, and individual effects are likely overestimated in the absence of other aerosols that can interfere with radiation. How has this been accounted for?

Similarly, the YIBs setup for assessing the clear sky impacts on GPP should be explained in detail. Can YIBs be run with clear sky radiation only? And if this is done simply by assuming year-round clear sky impacts, how do you account for the changes in the vegetation state (e.g. change in LAI) that are simulated as a result of the additional growth? Please explain how clear-sky effects have been assessed in YIBs. Also, other forcing than meteorological should be described, notably the role of CO2. If observed CO2 concentrations have been used, this might explain (part of) the trend in GPP displayed in Fig. 5ab.

Lastly, the manuscript is nicely written, but it heavily relies on material published in the supplementary information. I think that it is generally a good idea to provide supplementary material, but in the current manuscript, it is often "need to have" (rather than "nice to have") material that is in there. This results in a manuscript with a very large number of references to the supplementary information. I would recommend considering whether the text can be understood without access to the figures in the supplementary information. For those figures where this is not the case, I suggest promoting the figures into the main text.

Given these shortcomings, I cannot recommend the current study for publication in ACP, but I would like to encourage the authors to address these in a revised version of the manuscript.

Minor remarks:

- L. 28: consider replacing "ratio" by "fraction"
- L. 42: I do not agree with this explanation of the DFE! Enhanced GPP is not so much an effect of changes of the LUE of shaded leaves, but rather an increase of the fraction of shaded leaves (that have a higher LUE than sunlit leaves because of the lower PAR levels), hence increasing the total canopy LUE.
- L. 119: Please explain here why you do two sets of simulations (one with CEDS, and

one with EDGAR) – it is not clear why you need two alternatives, and how you will treat them in the manuscript.

- Eqs. (1) and (2): The computation of sunlit fraction from Beer-Lambert's law implies that not all light will be absorbed by a canopy, and in particular with low L, Fsunlit+Fshaded could be considerably lower than 1. Also, please clarify whether the described treatment of sunlit and shaded leaves is standard in YIBs or whether it was altered for this study.
- L. 208ff: Please provide more information in the main text about the observations, so that it can be understood without the supplementary information at hand. Specifically, please explain how the correlation coefficients and NMBs were determined (L. 210, 218, 219, etc): Is it based on spatial variations or temporal, and if the latter is considered, at which timescale (monthly?) is the temporal variation assessed? What statistical test was used to determine R values?
- L. 216: It is no surprise to obtain high R values when comparing simulated and observed magnitudes of SW radiation when considering that there is a clear latitudinal dependency that is present already in the top-of-atmosphere estimates. In order to assess the impacts of the radiative transfer scheme specifically, the authors may want to choose a more specific metric to evaluate its performance, e.g. the fraction of diffuse radiation.
- Fig. 2: Please consider using the same colour scale for all panels in the figure.
- L. 318: Replace "grids" with "of the grid cells"
- L. 331: Please mention the statistical test used to determine significance of the trend. Also, are numbers used for DFE here expressed as changes in GPP?
- L. 446-448: Consider replacing "cloud" by "clouds"
- Fig. 4cd: I am not so fond of the computation of the contribution of individual drivers based on absolute amounts – I think it is important to stress (also graphically in the figure) that BC has a negative, and all other aerosols a positive impact on GPP. See also my comments above about the computation of these contributions.