Re-Review of Yue et al., ACPD

"Ozone and haze pollution weakens net primary productivity in China"

I appreciate the authors' efforts to address the review comments. I would like to see a little more information on some of the points raised and I include some additional minor points below.

3. The paper needs a more consistent time-scale. The overall results are presented as annual, however all the figures (except Fig 10) show summertime results only. The authors should either include evaluation for all seasons (or annual means), or present the final results only for summer. As is, the reader cannot judge model skill or response for other seasons.

➔ The reason why our analyses and the Figures focus on the summer is that both GPP/NPP and air pollution (especially O3) reach maximum at this season. The largest interactions between carbon flux and air pollution are found for this season. It is not a contradiction to show Figures on the summer average and provide annual average impacts because the carbon loss in summer largely dominates the annual total. We found that, for O3 damages, "about 61% of such inhibition occurs in summer, when both photosynthesis and [O3] reach maximum of the year." (Lines 409-410). For the combined O3 and aerosol effects, "a dominant fraction (60% without AIE and 52% with AIE) of the reduced carbon uptake occurs in summer, when both NPP and [O3] reach maximum of the year." (Lines 474-476). We also elect to present and summarize the annual average results to the reader for consistency with regional carbon budget studies. Having the annual average values easily available facilitates comparison with other carbon flux impacts and carbon emissions. For example, we found that: "the combined effects of O3 and aerosols (Table 3) decrease total NPP in China by 0.39 (without AIE) to 0.80 Pg C yr-1 (with AIE), equivalent to 9-16% of the pollution-free NPP and 16-32% of the total anthropogenic carbon emissions". (Lines 469-471)

The authors themselves state that some of the results depend on the season (line 488) and this should be fully discussed. The authors may prefer to focus on summer in the main text Figures, but given that the impacts of other seasons are not negligible and the main conclusions are given as annual means, they MUST provide additional model evaluation in non-summer seasons in the Supplementary Materials. This should include evaluation of PM (AOD), O3, and radiation (Figures 1, 2, 3, 4). The results of this evaluation (consistency or not with summertime evaluation) should be briefly discussed in the main text.

4. The paper should discuss the potential implications of the high bias in simulated diffuse fraction and potentially in O3 (the evaluation of simulated O3 is mixed).

➔ We added following statements to discuss the implications of biases in diffuse fraction and O3: "Predicted [O3] is largely overestimated at urban sites but exhibits reasonable magnitude at rural sites (Figs 2 and 3). Measurements of background [O3] in China are limited both in space and time, restricting comprehensive validation of [O3] and the consequent estimate of O3 damages on the country level." (Lines 595-598)
"The model overestimates diffuse fraction in China (Fig. 4), likely because of simulated biases in clouds. Previously, we improved the prediction of diffuse fraction in China using observed cloud profiles for the region (Yue and Unger, 2017). Biases in simulated AOD and diffuse fraction introduce uncertainties in the aerosol DRF especially in the affected localized model grid cells. Yet, averaged over the China domain, our estimate of NPP change by aerosol DRF (0.09 Pg C yr-1) is consistent with the previous assessment in Yue and Unger (2017) (0.07 Pg C yr-1)." (Lines 619-625)

A follow-up question. As the authors emphasize that rural sites are more appropriate for evaluating their simulation, it seems reasonable to ask what fraction of the GPP change induced by pollution occurs over "urban" gridboxes? They suggest on line 541 that the change in GPP mainly derives from Eastern China, which is largely urban. This would provide some guidance as to how to interpret the relative urban vs rural O3 simulation bias.

6. Line 202-203: do these changes in biomass burning emissions seem realistic?

→ The reviewer raises an interesting and provocative question. The future changes in biomass burning in China are small, and that is indeed realistic based on current understanding of fire activity in China today. For example, wildfire activity is limited in China today. We state in the manuscript: "Biomass burning emissions, adopted from the IPCC RCP8.5 scenario (van Vuuren et al., 2011), are considered as anthropogenic sources because most fire activities in China are due to human-managed prescribed burning. Compared with the GAINs inventory, present-day biomass burning is equivalent to <1% of the emissions for NOx, SO2, and NH3, 1.6% for BC, 3.0% for CO, and 9.6% for OC." (Lines 213-217)

New sentence on fire activity in China being anthropogenic needs a literature reference.

8. Lines 208-209: Please explain why isoprene emissions increase and monoterpene emissions decrease (text later indicates that land cover is fixed)

→Please see above response to Point (7).

This is still a little unclear. Is the difference in emissions response for MT and ISOP emission to CO2, T, GPP (are MT emissions sensitive to GPP?) due to very different sensitivities to these factors, or due to geographical factors (i.e. regions dominated by MT see larger changes in CO2 than T, etc.)

12. Section 3.1.2 & Figure 2: Please briefly discuss where the model is too high and too low and what species might contribute to these biases. Also quantify the last sentence (line 298-299)

→We describe the AOD biases as follows: "Predicted AOD also reproduces the observed spatial pattern, but underestimates the high center in NCP by 24.6%." (Lines 339-340)

In the Discussion Section 4.2, we explain the cause of AOD biases: "Simulated surface PM2.5 is reasonable but AOD is underestimated in the North China Plain (Fig. 2), likely because of the biases in aerosol optical parameters. Using a different set of optical parameters, we predicted much higher AOD that is closer to observations with the same aerosol vertical profile and particle compositions (Yue and Unger, 2017)." (Lines 615-619)

We revise the text as follows: "Evaluations at rural sites (Table S4), which represent the major domain of China, show a mean bias of -5% (Fig. 3). The magnitude of such bias is much lower than the value of 42.5% for the comparisons at urban-dominant sites (Fig. 2f)." (Lines 346-348)

Is there any particular aspect to the "different set of optical parameters" that improves the simulation (i.e. scattering, absorption, water uptake, etc.)? Why did the authors not then use these superior aerosol optical parameters? A description & citation for current optical properties should be added to the Model Description.

Additional Points

1. Lines 340-341: It's not clear what this new text means. Was the baseline meteorology adjusted by these scaling factors? For each grid box? Please clarify/expand the description of this procedure.
2. Lines 360-366: line 360 indicates that NPP and GPP biases are less than 20%, but then specific biases of 23.7%, 20.6%, 40.0%, 51.2%, and 38.7% are not consistent with this. Please correct this text.
3. Section 3.1.3: The overestimate of diffuse fraction (line 398) seems likely to be associated with clouds (this is stated later in the text) given that aerosols are, if anything, underestimated. Have the authors compared the simulated clouds with other observational datasets? How do MERRA and the online clouds compare?
4. Figures 6, 8, 9, 10: could the authors indicate whether the local results are significant compared to interannual variability (as In Figure 7)