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.

This study explores the impact of air pollution on crop production, with a specific focus on China. This is a nice study and in many ways ambitious in scope, though it builds on a series of YIBs model developments described in previous literature. This is a great application of coupling atmospheric chemistry and biosphere modeling and in general I found the paper was well executed. I suggest a little more work to clarify the details behind these results, but after these minor corrections, the paper should be in good shape for publication in ACP.

Specific Comments

1. The paper would benefit from a clearer distinction/discussion of impacts attributed to meteorology feedbacks from PM & O3 forcing vs. aerosol indirect effects. The former are referred to as "direct effects" though they are in fact meteorological feedbacks. In general, it would be helpful if the authors provided a clearer quantification of these specific effects and the model simulations used to assess them.

→ We appreciate that the terminology may be a little challenging in multidisciplinary studies, but we believe that our decisions have made the impacts as clear as possible. We have adopted the use of "direct" and "indirect" as exactly used in the IPCC assessments because these terms are widely used in the aerosol-climate community. The direct and indirect aerosol effects are both associated with meteorological feedbacks. Throughout the manuscript, we emphasize when we are referring to feedbacks and whether they derive from aerosol direct and/or indirect effects.

2. The meteorological & hydrological responses presented primarily in 3.3 should include some standard deviation numbers since multiple years of simulation were run to assess natural variability. Are the changes in soil moisture and precipitation significant?

→ We have separated the original Table S6 into two Tables, with S5 for annual statistics and S6 for summer statistics. Each Table includes the mean changes and one standard deviation (brackets) indicating the uncertainties.

In Section 3.3, we have added following statement to emphasize that the changes in hydrological fields have large uncertainties: "Compared to aerosol-induced perturbations in radiation and temperature, responses in hydrological variables (e.g. precipitation and soil moisture) are usually statistically insignificant on the domain average due to the large relative interannual climate variability (Tables S5 and S6)." (Lines 428-431).

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 O_3 damages, "about 61% of such inhibition occurs in summer, when both photosynthesis and [O_3] reach maximum of the year." (Lines 409-410). For the combined O_3 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 [O_3] 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 O_3 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)

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 [O_3] is largely overestimated at urban sites but exhibits reasonable magnitude at rural sites (Figs 2 and 3). Measurements of background [O_3] in China are limited both in space and time, restricting comprehensive validation of [O_3] and the consequent estimate of O_3 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)

Details

1. Line 71: typo "meteorology, and clouds."

→ Corrected as suggested.

2. Line 90: need to define the square brackets in [O3]

→ We added the following definition: "…O_3 concentrations ($[O_3]$)" (Line 94).

3. Line 93-94: language "less well understood"

→ Corrected as suggested.

4. Line 149-150: not quite true, the CLM includes more PFTs, this should be clarified here.

→ The 8 PFTs used in climate model ModelE2-YIBs are aggregated from a land cover data set with 16 PFTs, which are used by the CLM model. We clarified as follows: "For both global and regional simulations, 8 plant functional types (PFTs) are considered (Fig. S1). This land cover is aggregated from a dataset with 16 PFTs, which are derived using retrievals …. The same vegetation cover with 16 PFTs is used by the Community Land Model (CLM)" (Lines 150-155).

5. Line 188: this is a large difference in NH3 emissions, do the authors know why the inventories differ?

→ We clarify as follows: "The discrepancies among different inventories emerge from varied assumptions on the stringency and effectiveness of emission control measures. While the GAINS 2010 ammonia emissions from China are larger than the RCP8.5 and HTAP emissions as shown in Fig. S2, they are close in magnitude to the year 2010 emissions of 13.84 Tg yr^{-1} estimated by the Regional Emission inventory in ASia (REAS, http://www.nies.go.jp/REAS/)." (Lines 193-198)

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 NO_x, SO_2, and NH_3, 1.6% for BC, 3.0% for CO, and 9.6% for OC." (Lines 213-217)

7. Lines 205-207: are these natural emissions simulated online or specified? Are there appropriate references that could be cited for this?

→ We add a detailed description of the climate-sensitive natural emission sources:

"The model represents climate-sensitive natural precursor emissions of lightning NO_x, soil NO_x and biogenic volatile organic compounds (BVOCs) (Unger and Yue, 2014). Future 2030 changes in these natural emissions are small compared to the anthropogenic emission changes. Interactive lightning NO_x emissions are calculated based on the climate model's moist convection scheme that is used to derive the total lightning and the cloud-to-ground lightning frequencies (Price et al., 1997; Pickering et al., 1998; Shindell et al., 2013). Annual average lightning NO_x emissions over China increase by 4% between 2010 and 2030. Interactive biogenic soil NO_x emission is parameterized as a function of PFT-type, soil temperature, precipitation (including pulsing events), fertilizer loss, LAI, NO_x dry deposition rate, and canopy wind speed (Yienger and Levy, 1995). Annual average biogenic soil NO_x emissions increase by only 1% over China between 2010 and 2030. Leaf isoprene emissions are simulated using a biochemical model that depends on the electron transport-limited photosynthetic rate, intercellular CO_2, canopy temperature, and atmospheric CO_2 (Unger et al., 2013). Leaf monoterpene emissions depend on canopy temperature and atmospheric CO_2 (Unger and Yue, 2014). Annual average isoprene emission in China increases by 5% (0.39 TgC/yr) between 2010 and 2030 in response to enhanced GPP and temperature that offset the effects of CO_2-inhibition. Monoterpene emissions decrease by 5% (-0.25 Tg C) between 2010 and 2030 because CO_2-inhibition outweighs the effects of increased warming." (Lines 220-238).

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).

9. Section 3.1.1 & Figure 1: Please discuss the spatial differences between observed and simulated GPP/NPP.

→We add the following information to Section 3.1.1: "For GPP, prediction in the summer overestimates by 6.2% over the southern coast (< 28°N), but underestimates by 23.7% over the North China Plain (NCP, [32-40°N, 110-120°E]). Compared with the MODIS data product, predicted summer NPP is overall overestimated by 20.6% in China (Fig. 1f), with regional biases of 40.0% in the southern coast, 51.2% in the NCP, and 38.7% in the Northeast (> 124°E)." (Lines 327-331)

10. Line 282: Is R=0.86 a typo? Figure 1 suggests this should be 0.75

→ The R=0.86 is for the annual GPP as shown in Figure S4. We have indicated both Figure 1 and Figure S4 in the text. (Line 325)

11. Figure 2 caption: should include years

→ We added the information of years as follows: "…observations from (b) the satellite retrieval of the MODIS averaged for 2008-2012), and (e) and (h) measurements from 188 ground-based sites (at the year 2014)" (Lines 959-961)

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 $PM_{2.5}$ 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)

13. Line 308: "diffuse fraction agree" – this is incorrect. The simulation appears biased quite high in some regions. Please correct.

→ We corrected the sentence: "Simulated diffuse fraction reproduces observed spatial pattern with high correlation coefficient (r = 0.74, $p < 0.01$), though it is on average 25.2% larger than observations (Figs 4d-4f). Such bias is mainly attributed to the overestimation in the North and Northeast. For the southeastern region, where high values of GPP dominate (Fig. 1), predicted diffuse fraction is in general within the 10% difference from the observations." (Lines 357-362)

14. Section 4.2 should also acknowledge that the response of the hydrological cycle to aerosols is also a major source of uncertainty.

→ We revised the text to acknowledge the uncertainty as follows: "Aerosol-induced impacts on precipitation and soil moisture are not statistically significant over the regionally averaged domain (Tables S5 and S6). However, for the 2010 and 2030 CLE cases with AIE, 2 out of 6 scenarios, the aerosol-induced impact on soil moisture dominates the total NPP response (Table 3)." (Lines 626-629)