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Discussions

Interactive comment on "Ozone and haze pollution weakens net primary productivity in China" *by* Xu Yue et al.

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The paper by Yue et al. is a valuable assessment of effects of ozone and aerosol pollution on NPP over China. In particular this is the first time the aerosol contribution has been examined in such detail. The modelled ozone damage is compared against field measurements. This paper should certainly be published in ACP, however some revision is needed as described below.

The authors should make it clearer how much of the impact of ozone and aerosols on NPP is natural and how much anthropogenic. The headline numbers are referred to as due to air pollution, but presumably there would be effects on NPP due to natural ozone. Two extra runs G10NATLO3 and G10NATHO3 would provide the required data for this. The authors assert that since the NPP effect is small below 40 ppb the no ozone and

natural ozone simulations are equivalent, but the authors need to demonstrate this with these two extra runs.

When analysing the meteorological changes the authors only show the impacts over China. In their global model set up the aerosols will change globally and affect global circulation (even with fixed SST). The authors should therefore show global maps corresponding to figures 7 and S8 in the supplement. One feature of perturbing aerosols in fixed-SST simulations is that there are large changes in the land-sea temperature contrast and hence artificial changes in circulation patterns. The resulting meteorological changes over China will therefore be a combination of locally driven effects (such as change in radiation and hence evaporation) and regional-globally driven effects (such as changes in rainfall and hence soil water). This seems to be particularly apparent in the AIE simulations where the patterns of changes in precipitation and soil water bear no relation to the changes in aerosol. The soil moisture changes dominate the aerosol impacts on NPP and I am not convinced these can be attributed to the aerosol changes. The changes in PAR and surface temperature can be much more readily linked physically to the changes in aerosol, therefore the authors should exclude the soil moisture changes from their analysis in table 2.

More explanation of table 2 is needed. What simulations are compared against what to derive the answers? How are the uncertainties derived? – presumably they are interannual variability, but different sets of annually-varying data are used for the online and offline calculations.

Specific points

Page 2, lines 47-53: Uncertainties (including the range between high and low sensitivity) need to be included here.

Page 2, line 55: suggest to replace "will not alleviate" with "will be further increased".

Page 4, line 93: "not been properly validated" – The authors need to be more explicit about exactly what deficiencies the previous studies had.

Page 7, line 186: The authors should clarify that they are referring to 2010 emissions.

Page 7, line 201: How much does biomass burning contribute to the emissions? Are they considered natural (in G10NATxxx)?

Page 7, lines 205-210: The authors need to describe how the changes in natural emissions are determined.

Page 8, line 217. I suggest including the table of simulations in the main text rather than the supplement.

Page 8, line 232. The authors should list the changes in WMGHG from 2010 to 2030 (at least CO2 and methane).

Page, 9 section 2.4.3. This section isn't clear about how the meteorological changes are applied to the offline model. Are they applied as an average (of the last 15 years) of the table S2 simulations; or are individual years from the table S2 simulations used as input. If the former: why is there any variability in the offline output? If the latter: the last 3 years could be strongly influenced by interannual variability.

Page 10, lines 298-299. The agreement in figure 3 doesn't suggest that the "Evaluations at rural sites better match the observations". The correlation is no better than for all sites, and by eye only the summer points look to show any correlation at all.

Page 12, line 346-348. The online model presumably allows the g_s changes to feed back on the ozone concentrations, which should increase them. Therefore it might be expected that the online model would show more ozone damage. The authors should compare the g_s and surface ozone concentration changes between the online and offline models.

Page 12, lines 348-349. Are the authors saying they have carried out a G10NATLO3 simulation, and the NPP change (compared to G10NATNO3) is identically zero every-

where? If so, this needs to be explained more clearly. If not, then the authors need to be clearer about the evidence they have that the zero anthropogenic emissions show no damage. The damage functions in fig 5 don't go exactly to zero at 40 ppb.

Page 12, line 352. The uncertainty here also needs to include the uncertainty in plant sensitivity (i.e. the range from high to low). Technically you should refer to the "central value" between high and low, rather than "average".

Page 12, line 362. Have the authors checked whether the absolute relative humidity is affected, i.e. whether the relative humidity change is purely due to the decreased temperature.

Page 12, lines 363-364. There are a lot of statements presented here without any evidence. The authors have not shown strengthened plant transpiration and have not demonstrated that any increase in RH is due to this (rather than simply decreased temperatures or increased horizontal moisture transport). Similarly the authors have not shown diagnostics demonstrating a direct causal chain between transpiration and precipitation or cloud cover. Both of these could instead be due to changes in circulation patterns.

Page 12, lines 367-369. Again no evidence is presented that the decrease in summer precipitation is due to a reduction in the cloud droplet size. Ultimately precipitation is driven by moisture convergence.

Page 13, lines 382-384. This sentence wasn't very clear. Is it referring to changes in heterotrophic respiration? If so, it should be said explicitly.

Page 13, line 395. There doesn't seem to be any change in soil water in the North China Plain (fig S8f) in the same region where NPP decreases in fig S9d.

Page 14, line 404. Is the agreement between the offline and online O3 inhibition true as a geographical pattern as well as the China total?

Page 14, line 406-407. Explain that the range quoted is for no AIE compared to AIE.

Uncertainties should also be quoted and include the range between high and low sensitivity.

Page 14, line 427. What is the change in methane in 2030?

Page 15, lines 434-436. It would be useful to be told the change in g_s between 2010 and 2030.

Page 15, line 436. Need to include the high-low sensitivity range here.

Page 15, line 441. Need to include the high-low sensitivity range here.

Figure 3. The right hand plot needs a legend to explain the colours.

Figures 4, 6, 8, 9, 10, S5, S7, S9 : The south east China box should be shown in every panel.

Figure 4. The key for blue and black dots should be provided within the graphs.

Figure 5. The key for colours should be provided in the graphs. It would be useful to provide the letter keys within table S1. A different key may be better as there a several authors starting with "Z".

Figure 8. Clarify here and/or in the text that the PAR changes include the DRF.

Figure S3. The key for colours should be provided in the graphs.

Figure S5. The colour scale for percentages should use the red colours for all the positive values, and blue only if there are negative ones, otherwise use the same colours as for the absolute values.

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