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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Projected effect of 2000–2050 changes in climate and emissions on aerosol levels in China and associated transboundary transport" *by* H. Jiang et al.

Anonymous Referee #2

Received and published: 7 May 2013

This manuscript estimates the impact on ozone and aerosols over China in 2050 compared to 2000, and tries to separate the impacts of emission changes and climate change.

Understanding and presenting the future impact of climate change and emission changes on different regions is important for policy makers, so this is an important area of research.

I have not seen the collection of observation data over China in the supplementary section before, so this could be valuable by itself.

My major concern with the work presented is that only 3 years of simulation is used to

calculate the effects of climate change, which I expect means that internal variability will be a large component of the climate change signal they use.

Note: because of the limited number of years I did not carefully read the detailed claims in manuscript.

I recommend several other significant changes too, detailed below.

The quality of the written English is excellent.

Overall, this manuscript is potentially a good paper, so I recommend major changes be made before publication.

— MAJOR RECOMMENDATIONS —

1. I expect that many of the spatial and seasonal climate change results presented are strongly affected by internal variability. I recommend either a) the simulations are extended for many more years (dependent on the type of ocean model used) or b) the interannual variability in the model is analyzed to prove that this is not important (3 data-points per location and season will give poor estimates of variability, but should be enough to give a rough estimate).

2. I recommend specifying the type of ocean model used in the climate model (specified sea-surface-temperatures, slab ocean model, full dynamical ocean model). This has a large impact on the interannual variability in a climate model, and hence the length of simulation required.

3. The role of ozone in the manuscript is unclear: is the manuscript about aerosols, or aerosols and ozone? It is not mentioned in the title, but it is discussed in the background section, briefly in the validation section, and briefly in the results sections, but not the boundary transport section. As I am sure the authors are aware, ozone is complicated because it depends on the non-linear balance between several different species and cloudiness (that affects photolysis), among other factors. However, the manuscript has only a cursory discussion and presentation about ozone. I suggest

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the authors consider whether to a) expand their material on ozone, b) delete the ozone sections and focus on aerosols, or c) explain the inclusion of ozone (eg, to help validate the model). I also suggest the authors consider whether to add “ozone” to the title.

4. A lot of the data is effectively presented in tables. I therefore suggest that it is not necessary to repeat information from the tables in the main text, unless it is being used to make a specific scientific point. This will significantly shorten the manuscript.

5. The changes in emissions and estimated concentrations are both presented. Since the concentration of many species is strongly driven by its main emissions, extra scientific value would be gained by directly comparing the changes in emissions and estimated concentrations to show the extent to which the change in concentration is non-linearly related to the change in the main emission for each specie.

6. I found no mention in the manuscript for how methane was handled in the simulations. This will be important to understanding the simulation results, especially for ozone and secondary aerosols. Since methane is also a greenhouse gas, I recommend that it be described how methane was handled for both a) radiative forcing in the climate simulation and b) the chemistry reactions.

7. Section 6 (conclusion and discussion). This section seems to be mostly a summary of the facts and figures presented in the tables and elsewhere. I recommend this section be significantly shortened, and be used to highlight the main scientific understanding gained from this work. The authors might also consider dropping ‘discussion’ from the section title.

8. Figure 6 shows that the model aerosol concentrations are often significantly different to observations. I recommend that the authors comment on how this impacts their conclusions.

9. Figure 10. I would normally expect boundary planes to form a box to help close the budget. Hence I would like to see an explanation for why the boundaries are disjoint. I

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am particularly interested in why the planes do not cover the India border when section 4.2 indicates it is important for changes over the Himalayan Plateau

10. The supplementary material with the aerosol observations over China is potentially very valuable. It would enhance the paper to provide some details on how to use it. For example: how far above the ground are they measured? What time of day are the measurements made? Are they hourly averages? daily averages? Could the data be provided with the paper in a spreadsheet file or some other data format?

— MINOR SUGGESTIONS —

1. Consider deleting 'and associated transboundary transport' from the title to shorten it.

2. Delete 'to' on p6503/line 25.

3. On p6504/lines 7-8, put the list of aerosol species in brackets rather than commas.

4. Often the change in aerosols is only given in ug/m3. I suggest the authors consider adding the percentage changes too.

5. p6506/line 18, I think it should be 2051.

6. p6506, line 20, I suggest adding 'the' before 'present study'.

7. p6507, line 11, I suggest changing the '/' symbols to commas.

8. I suggest moving section 2.4 to immediately after section 2.1 (or including it in section 2.1).

9. p6514, lines 3-7. Is it possible the increase in aerosol could be caused by increased natural emissions?

10. Section 5. Over what altitude range was the transboundary flux calculated? If it includes the stratosphere, then it would be good to comment on the effect this has (if any).

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11. Section 5. Is the transboundary flux calculated within the model every timestep, or is it calculated using time averaged winds and concentrations in the output?

12. P6526, lines 1-3. I find this sentence ambiguous. Does it mean 'climate change is important to air quality and long-range transport', or 'climate change and long-range transport are important to air quality'? I suggest rephrasing this sentence.

13. p6534, line 1. I suggest adding 'anthropogenic' before 'emissions'.

14. The acronym 'NMVOC' does not appear to be defined in the manuscript.

15. Table 2. The acronym 'HC' does not appear to be defined in the manuscript.

16. Tables 3-5. I assume the units should actually be Tg/year. It would also help to clarify whether the data is for the mass of the whole specie, or just one element (as is done in tables 1-2), eg Tg(S)/year, or Tg(SO4)/yr.

17. Fig 3a. It would help to clarify whether the cloud fraction is a true fraction (1=total overcast cloud) or a percentage.

18. Supplementary tables. I suggest clarifying the aerosol sizes of the data (I think they are all PM2.5).

— END OF COMMENTS —

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 6501, 2013.