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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Multi-model simulations of aerosol and ozone radiative forcing for the period 1990–2015" *by* Gunnar Myhre et al.

Anonymous Referee #2

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This study provides an assessment of the evolution of ozone and submicron aerosol atmospheric composition over the 1990-2015 period based on 7 global models and a new emission inventory from the EU ECLIPSE project. This time period is important in global change science for several reasons, including the possible "hiatus" in the global SAT record, and the large changes in regional pollution emissions (decreases in NH mid latitudes and increases in lower latitudes). The study provides global annual average radiative forcing diagnostics and surface concentration changes over the period. The main conclusion is that combined ozone and aerosols changes contributed a net positive global radiative forcing of about +200 mW/m2 between 1990 and 2015. The stronger net positive forcing than that reported in the IPCC AR5 is due to (unexplained) doubling of the ozone forcing, more stringent SO2 reductions and higher BC increases in ECLIPSE, relative to the previous IPCC emission inventory. The paper is clear and

well-written and merits publication in ACP once the following technical issues have been addressed.

1. This study assesses only the effects of anthropogenic emissions changes on the short-lived climate pollutants between 1990-2015. For example, the effects of other global change drivers including physical climate change and land use land cover change are not accounted for in the experimental protocol. The omission of these key drivers may be problematic given that the computed global forcings are quite small. New work from other groups and multimodel assessments is already indicating that physical climate change may be an important driver of chemical changes over this period. At the very least, the title needs to reflect that only changes in anthropogenic pollution emissions are examined and some discussion of the importance of other global change drivers (and why they have or have not been included) needs to be provided to help the readers.

2. A corollary is that the 7 models are based on entirely different chemical and meteorological background states/years (e.g. 2000, 2010 etc.) across the period and this probably represents an important part of the uncertainty ranges, but is not discussed at all. Some discussion and analysis needs to be added to the paper.

3. The paper includes an evaluation of simulated surface concentration trends against observational networks for the period. No measurement data for the entirety of Asia is included in the paper, which is not really acceptable these days, especially because a main focus of the study is on emission changes in Asia.

4. Backing up: Why is this evaluation against surface pollution concentration trends a part of this paper? What is the relationship between surface ozone and aerosol concentrations and their radiative forcings? Please explain. For ozone, the surface concentration change and global forcing changes are rather decoupled. The model/measurement surface ozone trend comparison given is not particularly convincing, and the quantitative details appear to have been relegated to the supplementary

information. Is this poor skill because the models in this study have simplified representation of land-atmosphere interactions? Would it be better for the specific goals of this paper to compare with global column and vertical profile measurements from the satellite records? E.g. MODIS, TES etc. Otherwise, I suggest including "surface concentration trends" in the paper title.

5. Table 1 needs sorting out because inconsistent terminology is used throughout. Please re-design the Table 1 with consistent terminology and acronyms e.g. N/A, 'yes', 'included'. What is L for EMEP? The models that used climatological SSTs and sea ice, for which decade/period? Monthly varying?

6. Table 1 indicates that the GISS model used '2000 met'. If I understand correctly, GISS is a coupled global CCM. There is an option to nudge to reanalysis winds but no full specified dynamics version is available? Please correct here, or provide a published reference for the specified dynamics version of GISS CCM?

7. Is it possible to provide an explanation for the doubled ozone forcing compared to IPCC AR5 value? Is it also due to the updated EU ECLIPSE emissions? The ozone radiative forcing section is very small compared to the aerosol sections! The paper can be improved and more interesting by presenting the major precursor drivers of the changes, and the reasons for discrepancies with other results.

8. I read several times over, and I find it difficult to understand exactly what is included in the multi-model mean "total aerosol forcing"? Can this definition be made clearer? I realize it is challenging in multimodel studies when models simulate different aerosol types and some represent aerosol-cloud interactions while others do not.

9. The uncertainty range needs to be added to the total forcing of +200mW/m2 in the abstract.

10. Would it be useful to add a comparison to the total CO2 forcing across this period? I believe the SLCP forcing is about 40% of the CO2 forcing across the period.

11. Page 4, Line 20 states: "Five models simulated surface ozone changes based on the prescribed emissions of precursors including methane." Does this mean that the models all have chemically dynamic ("flux-based") full methane cycle simulations? Or do the models prescribe methane atmospheric concentrations based on observed amounts? Should methane radiative forcings be included in this analysis? If the models are using flux-based methane simulations then more information is needed about the natural emissions and some solid evaluation of the simulated methane concentrations.

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