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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Historical and future changes in air pollutants from CMIP6 models" *by* Steven T. Turnock et al.

Anonymous Referee #2

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This manuscript conducts an evaluation of surface PM2.5 and ozone with observations for the CMIP6 chemistry-climate models that participated in AerChemMIP. It also documents the simulated historical & future changes in annual mean ozone and PM2.5 in various regions around the globe. It's clear that an enormous amount of effort went in to preparing this manuscript. By detailing the performance of each individual model (10 for PM2.5; 5 for ozone) against the available observations, a major community service has been performed in the production of this detailed supplemental information.

The rather long paper documents the current status of O3 and PM2.5 in the latest versions of global chemistry-climate models. It does so, however, without much attempt to understand more deeply the inter-model differences, or the sources of agreement, beyond discussing qualitative links to the emission trajectories or referencing relation-

ships identified in prior work. A stronger paper would be more cohesive throughout and communicate better the novelty of the work. Below I suggest ways to strengthen the paper in each of these two directions, followed by more detailed comments. I support the points made by the other reviewer and so try to avoid repeating those points here.

First, the model evaluation presented is not tied in a clear way to the past or future projections of the models. The evaluation focuses on monthly and seasonal data but then only annual mean concentrations are presented for the historical and future trends. It seems far more relevant to evaluate regional trends in annual mean concentrations where observations allow this, or to demonstrate some relationship between seasonal cycles and future changes across the models (and should one exist, this would be an exciting finding as it would open up the possibility of identifying a "best" model from the evaluation with observations). The evaluation shown in Figures 5 and 6 of the Mortier et al. paper or in Figure 4 of Griffiths et al. in this special issue seems more relevant, although the remote sites used in Griffiths et al. are not that relevant for the polluted regions examined in this study. One could tackle a similar type of evaluation for North America and Europe where there are at least two decades of long-term observations for ozone and PM2.5, and it should be particularly straightforward to do so with the gridded MERRA reanalysis product for PM2.5. An alternative angle could be to examine if the past or future trends are strongly seasonally dependent. If so, showing some of the seasonality in the projections would connect better to the seasonal evaluation included. If the authors choose to remove any of the current figures, they should be included in the supplemental material, as the general evaluation done here will certainly be of high value to the modeling community.

Second, the authors could better demonstrate the new contributions here, perhaps by looking a bit more closely at some aspect of the inter-model differences rather than ending with qualitative and in some cases speculative statements. For example, are there clear relationships between the inter-model spread in the global or regional temperature or precipitation changes and the air pollution changes projected over time?

Could previously identified general conclusions regarding relationships between global ozone, NOx and methane (see Figure 6 of Stevenson et al. 2006, Figure 13 of Young et al., 2013) be extended to surface ozone, and regionally? Can any conclusions be made as to whether future changes in particulate matter depend most on a particular component? There is a lot of useful information in the supplement regarding aerosol components and temperature changes that could be connected more closely to the changes reported in the main text. I find Figures 12 and 14 particularly interesting and the results presented there would be even more useful if they were connected more directly to changes in regional or global temperature, precipitation, humidity, air pollutant emissions, precursor surface concentrations, or whichever quantities are available across the set of models.

Detailed comments

One of the more interesting aspects of the paper is the comparison with the parameterisation based on HTAP models to separately attribute changes to emissions versus the combined emissions and climate changes simulated by the AerChemMIP models. However, it would help to have a better summary of how the parameterisation was developed and applied. Is it one parameterisation, or an ensemble of parameterisations that were developed separately for each model? Is there any overlap in the models used in developing the parameterisation and the AerChemMIP models? If so, can that subset of models be analyzed to attribute with greater confidence the role of climate change? Would this study support future work to extend this parameterisation to include the effects of temperature, humidity, or some other changes in climate variables?

The referencing throughout the text seems to focus on more recent work rather than early papers that first identified important relationships. For example, the role of increasing water vapor in increasing ozone loss was first pointed out by Johnson et al., 1999 (text around line 65, and especially 450); the role of methane for surface ozone by Fiore et al. 2002 and Shindell et al. 2012 (text around line 65); the increase in ozone under climate change scenarios by Wu et al. 2009 and Weaver et al. 2009 (text around

line 645).

Try to quantify wherever possible in the text, such as line 29 “consistent overestimate”, line 31 “consistently underestimated”, by how much? Is there any improvement in biases, or worsening, relative to prior studies? Line 40 “important differences”, can anything be said as to which is most important or handled most realistically? Line 44-45 should include at least one example to support this statement.

Lines 113-114. Why do this for a future scenario rather than the historical period where there might be some opportunity to evaluate with observations?

Figure 2 is difficult to digest. Why does this need to be in the main text? This is an example where more could be gleaned from the analysis if these changes in emissions could be shown to be related to the projected changes in ozone and/or PM2.5, perhaps through scatterplots.

Line 271. This can be checked and stated more confidently by examining NO2+O3 rather than just O3.

Lines 444-445 is not new as this was a major result from CMIP5 era RCP8.5. Some of that work probably deserves a citation, such as Gao et al. 2013.

The biases in Figure 3 are very hard to read. It should be stated if the color bar saturates.

Lines 494-500. These seemingly different responses may occur because of different responses in winter versus summer across the models being mixed together in the annual mean.

Lines 503-514. Can these points about sources of inter-model differences be illustrated and based on evidence rather than surmised? Same goes for lines 580-590 & 600-602, where it might be worth moving some of the supplemental information into the main text to support more strongly these points.

Lines 648-650 should be supported with observations for this conclusion to be made here.

Stronger evidence should also be included to support conclusions on lines 665-666 & 677-678.

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