Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2019-1216-RC1, 2020

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Tropospheric ozone in CMIP6 Simulations" *by* Paul T. Griffiths et al.

Mathew Evans (Referee)

mat.evans@york.ac.uk

Received and published: 13 March 2020

This paper evaluates the simulations of tropospheric ozone from the "preindustrial" (1850), through the present day, to 2100 undertaken as part of the CMIP6 chemistry climate model simulations. This should inform the next IPCC report, and is part of an ongoing multi-decadal project to provide this information for the IPCC reports. The timings for the submission of the paper is mainly driven by the IPCC timescales.

There is utility in publishing this paper. Having a new assessment of both the performance of the current generation of chemistry-climate models and their variability is useful. I would suggest publication after some changes.

There are however some disappointments inherent in this paper which are symptomatic of the CMIP process for tropospheric composition. The comments below are more directed to the wider CMIP community than the authors specifically. 1) The tropospheric

chemistry modelling community appears to be disengaging from this process. Looking at the ACCENT (2006), ACCMIP (2013) and the present study there is a linear decay in the numbers of models taking part in this tropospheric ozone budget aspect. Interpolation would suggest that there will be no models engaging in the process by around 2023. It would be useful for the CMIP community to consider why this is the case, and think about how the outside community is valuing its activities. 2) Papers very similar to this have been being published for the last decades. The authors refer to Young et al. (2013) and Stevenson et al. (2006) as the precursors to this, and there are previous activities which are very similar going back to chapters in the 2001 IPCC report and earlier. It is not obvious that the models' ability to simulate ozone is getting any better over this timescale. One of the conclusions from this paper is the present day mean O3 burden (ACCENT to CMIP6) has only changed by 3% from 15 years of research. 3) It is also of concern that the tools used to analyse these models has not changed in these almost twenty years and the explanations for model differences have similarly not evolved from being a combination of chemistry, emissions, deposition and transport. Perhaps the authors would want to consider whether there needs to be advances in diagnostic techniques before the next model comparison exercise in the conclusions / discussions?

These points are not issues associated with this paper specifically and the authors don't need to reply to these questions but it may be useful for the wider community to think about this.

Specific comments.

Relationship to other CMIP6 papers There are a number of papers submitted to a number of journals based on these CMIP6 simulations. It would be useful to provide some explanation in this paper as to where it is expected to sit in relation to the other papers. Is there a separate paper discussion stratospheric O3? Ozone radiative forcing? OH? CH4 etc. There is some nods to some of these papers but it isn't clear how this is likely to fit in with the other papers. Could the authors provide information about the other

papers currently going through review which touch on this topic (stratospheric ozone, OH etc).

Models 4 models are described in section 2. But I only see 3 models in figures etc. Section 2.1 says that the 'ozone evaluation' uses 4 models but the 'budgets' use 3. But it is unclear therefore why the GISS model doesn't appear in Figure 1,8 and 15. It is unclear which models are being included in which metrics. Could each metric please indicate whether it is calculated from the multi model mean of 3 or 4 models? As discussed earlier this is a small number, especially given previous evaluations. Could the authors give a little bit of context? Are there fewer models engaging in the whole CMIP process, or is it just this tropospheric chemistry aspect aspect? Was the minimum criteria for inclusion solely providing a tropospheric ozone concentration or were there others?

Model description It would be useful to have a table outlining the model configurations. Sections 2.0.1-2.0.4 give differing bits of information about the models and understanding what is the same and what is different between the models is difficult. There are only 3 or 4 models so it shouldn't be too difficult to pull the useful bits of information from the models on things like – resolution, anthropogenic emitted species, lighting emissions scheme, soil nox emissions, biogenic emissions schemes, treatment of aerosols, heterogenous chemistry in a standardized format etc.

Model's representation of tropospheric processes It would be useful for the authors to comment on whether these models represent our best understanding of atmospheric chemistry and, if not, what could the implications of this be. These models are by their very nature fairly conservative in what processes they include and their complexity of representation. But they likely miss some significant processes such as tropospheric halogens, and a complete representation of organic chemistry, heterogenous chemistry etc. It would be useful to have some comments (probably in the discussion) of what this might mean for the conclusions drawn here.

Future and past emissions It would be useful to have a description of the ssp370 emissions – what are the asssumptions about how the world gets to 2100? To those embedded in the IPCC process this might be obvious but to those who are not it is hard to know what this scenario is and what it assumes about the state of the world etc. It would also be useful to have a sense of how the results from these simulations compare to the world predicted by the previous round of model assessments with the RCPs. It would be useful to mark the multi-model mean O3 burdens for 2100 found from the last round of CMIP model experiments on Figure 8 for example. Similarily, how do the preindustrial anthropogenic emissions differ from those previously used in these assessments?

Conclusions The community has been around the cycle of IPCC reported model comparison exercises for tropospheric ozone multiple times now over the last two decades. Figure 8 shows that for the preindustrial to the present day the model prediction (well the multi-model mean) hasn't essentially changed since the ACCMIP evaluation. The explanation for the spread between these models also hasn't really changed. It is some indistinct combination of different emissions, chemistry, deposition and transport in the model. It might be useful to the community for the authors to provide a potential vision of how things should change going forwards. Will the CMIP7 version of this paper look exactly the same as this? If not how should we make advances in the future?

Acronyms There are quite a few acronyms used in the paper. These tend to alienate readers so it would be useful to see if some of them can be removed especially when they are only referred to a few times after being defined. Can the full wording also be used when the definition of the acronym is well away from its usage (on a different page etc). It took me a while to workout what BDC was on page 16.

Specific comments Abstract. Line 10. It's not clear whether the large differences between the models (30%) is referring to the burden or the budget.

Page 16. The OPE calculation is very interesting. This shows a much larger range

than I would have assumed. The authors argue that that it is the differences in the background VOC mixing ratio in the model to explain this. It might not be that simple and they don't really provide any evidence to support this. Differences in the chemistry schemes may play a role here in a number of ways. Choices about the chemical rate constants, mechanistic choices and the speciation of VOCs used could all cause differences They do show that there are differences in the bVOC emissions (Figure 1) but there isn't any other evidence to support their argument about this being due to background VOC mixing ratios (they discuss NMHVOCs in one sentence and then in the next use VOC; is in the other is there a subtly in their argument about CH4 here that I'm not getting). There are also substantial differences in the mean NOx concentrations being calculated which would also influence the OPE. Without some additional evidence the explanations of the model performance appear to be somewhat of a throw away comment.

Tables. Table 1+2 These table are currently without units. It would be useful to include some additional information. The ozone burden would be useful as would the mean lifetime (Burden/(L+DD)). The table seems quite long. Reporting fewer times would not change the story.

Figures.

Figure 1. Could this be expanded to include CH4 concentrations or emissions and the anthropogenic emissions of VOCs, SO2 etc? This would help to put the rest of the paper into context. It would also be useful to know what the models are predicting for OH concentration. I realise this that might be being covered in more detail in another publication but it is hard to understand the impacts of O3 without understanding the influence of OH. If there are other papers covering other areas of the model simulations it would be useful to understand which papers will be covering which activities.

Figure 2. Extra dot before C.E.

Figure 4. The markers are too small to see the colours. I'd suggest that they are just

filled back squares or circles. This seems to be discussed before figure 3?

Figure 5. Can you explain MMS, MMM in the caption text.

Figure 8. Can the models be described in the caption box as well as in the text. It would make it easier to understand.

Figure 14. It would be more useful to know the deposition velocities in the model than the fluxes here. In trying to attribute change it is hard to know whether it's the differences in the O3 concentrations calculated by the model which are causing the differences or the changes in the land surfaces or assumptions about land surface which are causing these differences.

Figure 15. I'm not sure that the units are appropriate here? The units say Tg(O3)/yr but shouldn't the model resolution be taken into account here? The text says that the models are at their native grid so a model at 2x2.5 resolution compared to one at 1x1 would have 5 times as much ozone production in each gridbox which would make it look much redder even if the integrated ozone production was the same? Similarily, will this plot also tend to over emphasise the poles in the budget as it given them equal weight on the plot as the tropics?

Figure 16. Can this be converted into two plots? One of ozone production and one loss? It is a bit busy at the moment.

Figure 17. Can the scale on the plots be changed to reduce the emphasis on the stratosphere and increase the emaphasis on the troposphere? 300 ppbv of O3 is pretty high? Page 16. There are a lot of acronymns here which don't I think make the document transparaent. BDC is defined much earlier in the document making understanding difficult.

Data availability. Can the ESGF be spelt out in more details and a website given?

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2019-1216,

2020.