Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2019-1202-RC2, 2020

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Evaluating stratospheric ozone and water vapor changes in CMIP6 models from 1850–2100" *by* James Keeble et al.

Anonymous Referee #3

Received and published: 20 March 2020

General Comment

This paper evaluated CMIP6 models in terms of their ability to simulate past to future variation in the stratospheric ozone and water vapor. The authors examined how well the CMIP6 models represent stratospheric ozone profile and past change in total column ozone (TCO) by comparing with the observation in global and regional perspective. They showed the considerably diversified future projections of stratospheric ozone with different SSP scenarios. The stratospheric water vapor in CMIP6 models was revealed to have a couple of problems for adequately simulating its observed features partly because some models ignore methane oxidation in the stratosphere. I fully acknowledge the importance of this work and it will provide a useful information to the climate science community. This paper is rightfully within the scope of ACP, however,

I noticed several issues in this paper which cannot be passed over to be published. I suggested that the authors should consider the following comments: two major and several specific comments.

Major Comment 1:

The authors relatively well described the difference among CMIP6 models, pointing out some models showing over/under-estimation. However, they only provided a limited discussion and description about possible reasons for such a spread among models. As a result, the current manuscript ended up being a superficial model intercomparison. I recommend the authors to spend more words to discuss why some models differ significantly from the other models. For example, could you discuss more about why UKESM1-0-LL model greatly overestimate the TCO, GFDL models underestimate, MRI-ESM2-0 showed quite a small temporal change in TCO from 1950 onward, and the like?

Major Comment 2:

There is several important information which were not properly provided in the manuscript.

[1] Description of CMIP6 models in chapter 2.1 should provide more unified information on each model. The current descriptions differed considerably between models. At least, 1) model resolution (horizontal and vertical), 2) treatment of ozone-related chemical process both in stratosphere and troposphere and 3) CH4 oxidation in the stratosphere should be provided for all models. Or it's better to include that information in Table 1.

[2] Many statistics used in the manuscript are not well defined. The authors should carefully describe the statistics in the manuscript and figure captions. Sometimes I could not understand what kind of temporal and/or special means were used in some figures, since the descriptions in the manuscript or figure captions are so rough and

blur. The lack of carefulness like this largely deteriorate readability and value of the manuscript. The author should carefully revise the manuscript and figure captions to provide sufficient description about the statistics used. I noticed several points in the following specific comments section.

Specific Comments:

- L80-81: Heterogeneous chemistry is also important.

- L99-100: Could you briefly describe how BDC control the oxidation of Cly, NOy and HOx species.

- L104: Why you didn't use the abbreviation "SWV" here? Please uniformly use the words which you dare to define throughout the manuscript.

- 2.1 Models:

[1] The models are described as "fully coupled", "online", or "interactive" chemistry. You should give precise description what these words mean. If there are no difference among them, you should use one specific word to describe it. I suppose all these words mean that calculated chemical species concentration is used in, so "coupled" or "interactive" with, the radiation calculation. Is it correct? Are there any model who calculate the chemical species "online" but they are not used in radiation calculation? Are there any models who only calculate stratospheric or tropospheric chemistry online but used prescribed concentration in the other sphere? Could you clearly describe these details of each model in this chapter or summarize in Table 1 ?(Please also see Major comment 2 [1])

[2] As to prescribed chemistry models, it should be clearly stated how these models treat the chemical species concentration in the model. I could not understand why these models output different ozone concentration as depicted in the Figures even if they prescribed the same CMIP6 dataset, although I know CESM2 used their original ozone data and so could output different ozone fields. This is one of the key points

for the readers to correctly understand this paper. Particularly, it should be described for each model whether the model prescribed concentrations entire model domain or only prescribed at the surface and allowed to calculate the atmospheric concentrations online.

- L152: Appendix shows the relevant "difference" among models but do not provide the "details" of each model.

- L273: How did each model force the historical changes in short-lived species (mainly air pollutants and its precursors) and long-lived GHG? Whether were they input as emission or surface concentration?

- L287: What are "low" SSPs ?

- L298-305: Any abbreviations should be spelled out at their first appearance, NIWA-BS, SWOOSH and satellite sensors names.

- L322: What is (10o) ?

- L343-345: CESM2 and FGOALS-g3 models showed larger overestimation than BCC-EMS1. Why did you particularly pick up these two (BCC-ESM1 and SAM0-UNICON) models here? Also, SAM0-UNICOM does not have peaks in the mid-latitude.

- Figure2: What is the shaded region?

- L358: It's hard to distinguish each model's line, so I'm not quite sure that I could tell BCC-ESM1 correctly, this model was not low-biased, but "negatively" high-biased. SAM0-UNICON model is also negatively high-biased.

- L362-363: Differences of the CMIP6 MMM from the observation described here in the lower stratosphere and over 1 hPa in the mid-latitudes are not mutual among all CMIP6 models. From Figure A1 it is clear that these differences are mainly owing to only a few models. So the author should describe here more carefully. Putting a figure of standard deviation among models together in Figure 4 might be an option.

- Figure3: It's better to use different color pallet to make it more easy to identify the difference among models.

- L380-381: Is the MMM TCO underestimation in SH polar region in polar winter real? The NIWA-BS data in this area in this season is mainly made by filling missing data as I correctly understand chapter 2.3 of the manuscript, so it might be artificial not real. Can you compare the MMM TCO with other data source, such as ground based TCO observation in SH polar region?

- Figure 5: Figures are a bit small and hard to recognize each symbol. Could you provide the detailed description how did you calculate statistics used in these Figures? It is not self-apparent what "spatial std dev" or "percentage bias" mean. There are several definitions to calculate those statistics. The descriptions can be in appendix or as a supplement material.

- L394-395: Why does a large "spatial" standard deviation for the SMA0-UNICON and MRI-ESM2-0 models indicate higher interannual, so "temporal", variability? (Please also consider my comment for Figure 5)

- L406-407: Why do only these two models show no interannual variability? How about other models who used prescribed ozone fields? (Please also see the comment for "2.1 Models" [2])

- Table2:

[1] The number of ensemble member for each model should be summarized in Table1. Moreover, it must be described somewhere in the manuscript how the ensemble member was treated in all the analysis for this paper. Did you use ensemble means for all the figure?

[2] What does "errors" exactly mean?

[3] Could you also provide the trend of observation (NIWA-BS) for 2000-2014?

- L411: What does "overall TCO decline" exactly mean here?

- L430: The modelled trends in TCO for 2000-2014 are small but not mostly non-significant.

- Figure7 (and for some other figures): Why did you use "standard error" not "standard deviation" for indicating the model spread? The standard deviation is appropriate for this purpose.

- L440: I could not see the TCO increase of "20-30DU" from 1850 to 1960 in NH in Figure 7. Could you revise the number?

- L440-441: English is too complicated for me to understand correctly what it means.

- L446: How did you evaluate the "ability" of models to simulate pre-industrial TCO? Since we don't have TCO observation in that era, we cannot ensure the model's ability through comparing model results with observation.

- L448-454: Could you make more discussion about the difference among the models in simulating the past TCO changes. Discussions on why the models prescribing CMIP6 ozone data showed such a large discrepancy and those on the overestimation of ozone decline by some models are desirable.

- Figure 8: Is the SSP370 scenario simulation result necessary for this figure. This part was never referred in the manuscript.

- L458: Typo. "TCO seen in Figure 8" -> Figure 7

- L460: Where did you describe about a large tropospheric ozone bias of UKESM1-0-LL in the manuscript? Which figure show that?

- L490: Is this for SSP1-1.9 scenario not for SSP1-2.6?

- L515: Could you add the changes in the BDC simulated in the CMIP6 models to Figure 10?

- L529-530: Figure A4 should be cited here if you want to refer to the percentage difference, since Figure 11 cannot show it.

- L535-536: How is the temperature at the tropical tropopause in the CNRM models? Is there any low temperature bias there which can cause the dry bias in the stratosphere in those models?

- L538" As for ?

- L555: What does "CH4" exactly means in this equation? Concentration? Mixing ratio? What is its unit?

- Figure 14: There are no reference to Figure 14 in the manuscript. The figure capture does not include the description of color bar. What does each point in the figure represent? Are they annual mean? Horrible lack of information for this figure.

- Figure 15: Why you did not comment anything on the comparison with the observation. The CMIP6 models apparently underestimate the observation and the modelled increasing trend in the stratospheric water vapor can not be seen in the observation. You should discuss about those comparison in the manuscript.

- L580-582: How is the temperature change at the tropopause not at the 100 hPa? Is the increase at 100 hPa temperature and the increase in water vapor quantitatively consistent with each other?

- L588-590: Could you show separately the relative contribution of 100 hPa temperature rise and CH4 concentration increase for the stratospheric water vapor increase in the CMIP6 models? Both for historical simulation and future projections.

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