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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Sensitivity of the southern hemisphere tropospheric jet response to Antarctic ozone depletion: prescribed versus interactive chemistry" *by* Sabine Haase et al.

Anonymous Referee #2

Received and published: 1 July 2020

Summary: I quite liked reviewing this paper. The authors systematically compared two configurations of the same chemistry-climate model (CESM1-WACCM plus some modifications) that differ in that one configuration has fully interactive ozone chemistry, whereas the other uses prescribed ozone fields generated by the same model. The authors also test the sensitivity to prescribing zonally symmetric versus zonally resolved ozone fields. They find that the two model configurations produce qualitatively similar results but with important quantitative differences, e.g. regarding the coupling of the polar vortex strength with ozone depletion, timescales of variability of the Southern Annular Mode, an acceleration of the westerlies in the Southern Hemisphere, etc. They find that prescribing zonally resolved ozone produces results that are generally closer

to those produced using interactive ozone.

The results are of interest to climate modellers weighing up whether to include interactive ozone in climate projection simulations. Often the additional computational cost and scientific effort needed to sustain this functionality are considered prohibitive. There is a small number of other papers that characterize the advantages of interactively simulating ozone, and often these papers involve comparisons that are not entirely balanced, e.g. by comparing groups of different models. There is thus clearly a niche for this paper that aims to quantify the differences between the two approaches with minimal interference from other model differences (that are not ozone). (The authors do not divulge any details about the computational costs of their three ensembles; maybe this small detail can be added.)

In a few places error bounds should be stated before an explanation is given for why two quantities are different. Otherwise we cannot be sure that such differences are not coincidental in nature. Haase and Matthes (2019) are cited extensively. Perhaps the authors could elaborate a bit more how the results produced here compare to the results shown in that reference. Do the differences come down to the same mechanism in both hemispheres?

I could not discern whether the model is in a coupled atmosphere-ocean or an atmosphere-only configuration. If it is coupled, do perhaps slow modes of oceanic variability influence the results (that perhaps evolve differently in the different ensembles)?

I also agree with another reviewer that the title should be revised given the balance of evidence presented in this paper. The SH tropospheric jet seems to be a relatively minor topic here. While the authors state that HM19 is about NH results, they are cited in reference to SH features too, so a bit more discussion about the key differences between the two papers would be good to have.

The language is mostly fine (some minor style issues are listed below), the figures are informative and about right in number, the conclusions are balanced. I thus recommend publication in ACP once my comments are addressed.

Minor comments:

L5 and line 63: I suggest to replace “accurate” with “appropriate”, “self-consistent” or similar. Eyring et al. (2013) showed for CMIP5 models that interactive ozone models can fail to produce “accurate” fields. . . (https://doi.org/10.1002/jgrd.50316)

L52: Whether ozone recovery will ever be stronger than GHG influences may also be a function of the assumed GHG scenario.

L62-72: A third, fairly widespread method is to use an online parameterization for ozone (i.e. make ozone interactive but not use comprehensive chemistry). This route is followed in several CMIP6 models, sometimes to the point that models with and without comprehensive-chemistry ozone schemes are almost indistinguishable in their performances (e.g. CNRM models). Given the results of this paper (showing that prescribing 3D ozone fields already constitutes progress) I’d say that for some groups this might be the way forward.

L89: Cut out “along”.

L153: Replace “as well as” with “like”.

L160: Replace “from the land fraction factor” with “on land fraction”.

L162-163: How do you know the cold-pole bias has been reduced in pre-industrial simulations? Almost nothing is known about the pre-industrial stratosphere. . .

L236: Replace “It was shown in Haase and Matthes (2019)” with “HM19 showed”.

L245: I don’t understand why the amplitude of the response should be smaller with a larger ensemble. The mean response should be better defined as the ensemble size decreases. Please expand / explain.

L330: Whether these numbers are indeed different requires some kind of analysis of the statistical uncertainties. If they are not different within their uncertainty bounds, the argument would fall apart.

L373ff: Note also https://doi.org/10.1002/2014JD023009 who also studied stratospheric SAM variability and found an increase in variability under ozone depletion.

L390ff: I don't think it's appropriate here to discuss an unpublished paper. If it has meanwhile been published (but perhaps not fully peer-reviewed) that would be OK. If not, I suggest to remove this section.

L399: I'm sure the "polar stereographic" map projections are not relevant here, but rather the physical quantities / questions that you want to address. Which fields are you assessing here?

L447: Please spell out code availability, a requirement for publication in ACP.

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