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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 #1

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This manuscript examines the impact of the representation of stratospheric ozone on climate model simulations of tropospheric jet trends, by comparing ensembles of simulation with (i) interactive chemistry, (ii) prescribed zonal-mean ozone, and (iii) prescribed 3D ozone. This is an important topic that is relevant for ACP, and the manuscript is generally well written and presents some new results. However, before it can be published there needs to be more, quantitative analysis of the differences in jet trends among simulations, as well as discussion of some relevant previous studies that have not been cited.

MAJOR COMMENTS

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1. The title indicated that the tropospheric jet is the focus of this study, but most of the focus is on the stratosphere and not the troposphere. Only one subsection of results is on tropospheric jet, only 2 out 9 figures show the tropospheric jet, and the first 1.5 pages of Introduction are on stratosphere and only at line 85 is surface/tropospheric features discussed. I think there should be more discussion and analysis of the tropospheric jet, and less material on stratospheric changes.

2. Regarding additional analysis, there are statements on how the shift in the jet differs between the ON, OF and OFF 3D runs (lines 364-370 and 435) but this is not quantified. The near-surface differences shown in the fig 9c and e and small (and generally insignificant), and it is not clear from these plots how different the jet trends are. As the tropospheric jet response is the focus not the paper trends in the latitude and strength of the tropospheric jet (e.g. u at 850 hPa) need to be calculated, and compared between different model runs (as well as reanalyses). Do the trends differ, and how large is the difference compared to model-data differences? This is important given the comment on lines 3 and 22 in the abstract (see minor comment 1), and also Seviour et al. (2017) (Major Comment 3).

3. Missing references

Several key references are missing.

Waugh et al. 2009 was one of the first (if not the first) studies to look at impact of interactive versus specific ozone on SH trends, and should be included at least in discussion on pg 4)

Seviour et al. 2016 compared runs with specified daily and month ozone (see, in particular, section 3b) and should be referenced. See also minor comment 2.

Seviour et al. 2017 compared different simulations of the tropospheric response to ozone depletion (including the results from the 2016 paper). This paper showed that the statistical uncertainty in tropospheric jet changes was very large, and although

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there were variations among simulations in the mean changes they all agreed within their uncertainties. Is this also the case for the 3 ensembles considered here?

MINOR COMMENTS

1. Line 3: "differ largely" and Line 22 "crucial for representing" both appear to be over-statements, both based on previous studies and this study. Yes there are differences depending on the ozone but I am not sure can be classed as large or crucial.

2. Line 10: "In contrast to earlier studies, we use daily-resolved ozone fields". This is not the first study to use daily-resolved ozone (e.g. Neely et al, Seviour et al. 2016).

3. Line 390, 445: Iyvanciu et al. (in prep). At the very least a paper needs to be submitted before it can be referenced.

4. Line 410: I don't understand what is meant by "The LW heating rate trend does not add more information".

REFERENCES

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