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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Future Antarctic ozone recovery rates in September–December predicted by CCMVal-2 model simulations" *by* J. M. Siddaway et al.

Anonymous Referee #1

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This manuscript describes an analysis of the recovery of Antarctic ozone in simulations of the 21st century by a collection of chemistry-climate models (CCMs). I think the manuscript contains some new and interesting results, but major revisions are required before it is suitable for publication. In particular, the discussion of the literature is extremely poor, and there are numerous studies that have done similar analysis that need to be referenced and discussed. These additions will also have to clearly identify what is new in this analysis (some of the claims of new results in this manuscript are incorrect).

MAJOR COMMENTS

1. Analysis of ozone recovery in the CCMVal-2 models is not new, and there are many previous studies that have done similar analysis to this manuscript that are not referenced. Further, even some of the referenced papers are not discussed in much detail.

Reference and discussion of these studies must be included in this manuscript. If a similar analysis has been done before it needs to be referenced and whether results are consistent discussed. Only when this is done can you claim new results.

A few examples are (which are all listed at http://www.pa.op.dlr.de/CCMVal/CCMVal_publicati

Austin, J., et al, 2010 focuses on exactly the similar topic as current manuscript, and uses the same model output. It shows time series of Antarctic TCO and Cly, plus analysis of return dates of both. E.g., Are your results any different?

Oman, L. D., et al, 2010 includes analysis and discussion of recovery of lower stratosphere and upper stratosphere Antarctic ozone, with comparison to TCO and Cly. E.g., Is your multi-level analysis consistent with the two layer approach used in Oman et al?

Butchart, N., et al, 2010 is analysis of CCMVal-1 simulations but still relevant as discusses projections of Antarctic stratospheric temperatures. E.g., Are the projections in CCMVal-2 similar to CCMVal-1?

There is additional analysis in the SPARC 2010 report, and the analysis in Eyring et al 2010 needs to be discussed more than it is.

Further, the above are not the only relevant papers, and there are single model papers that should also be referenced.

2. I think the choice of 1970-1979 is unfortunate, as previous studies have either used 1980 and 1960, so it is not possible to quantitatively compare your results with previous studies. I think paper would be improved if either 1980 or 1960 (or even 1960-1969) were used. The justification for 1975 (or 1970-79) given in the manuscript is weak.

3. There is no real discussion of the role of recovery of chlorine and bromine in the recovery of ozone. The above studies include this, and show this is a major factor (and I think more important than temperature and winds). I think analysis of EESC, or something like this, is needed to complete the analysis. Again, I think you need to build on what has already been done. Without figures showing the chlorine evolution a reader may get the impression that T and winds are the important factors driving changes in ozone.

4. There are some organizational issues with the manuscript. These are mainly the title of sections, and so could be easily fixed. However, as it is the titles are not consistent with their contents.

Section 2 is "Model description" but there is more than one model, and the models are not really described. How about Model Archive or Model Simulations.

Section 3 is Data Analysis and Discussion, but it is really the results section.

Section 3.1 is Methodology but half way through it you present results.

Section 4 is more a discussion plus conclusions, and includes a summary at the start and then near the end. This needs to more concise.

MINOR COMMENTS

Pg 18963, line 3: ". . . anthropogenic forcings . . . are not included for the past .." Incorrect, changes in GHGs and ODSs are included. Natural variations like solar and volcanic eruptions are not included.

Pg 18963, line 16: The discontinuity in GEOSCCM is I think before its REF2 started at 2000, and 1960-2000 is from a separate run.

Pg 18976, last paragraph: I don't like papers that end in a long wish list for things others should do. Also, there are several studies out there that do address some of these issues, and should be reference. In particular, the CCMVal-2 archive includes

runs with different GHG scenarios as well as fixed chlorine or fixed GHG runs, and there have been several papers on this that need to be referenced (e.g. Eyring et al. 2010b)

REFERENCES

SPARC CCMVal (2010), SPARC Report on the Evaluation of Chemistry-Climate Models, V. Eyring, T. G. Shepherd, D. W. Waugh (Eds.), SPARC Report No. 5, WCRP-132, WMO/TD-No. 1526, http://www.atmosp.physics.utoronto.ca/SPARC.

Austin, J., et al, The decline and recovery of total column ozone using a multi-model time series analysis, J. Geophys. Res., 115, D00M10, doi:10.1029/2010JD013857, 2010.

Oman, L. D., et al, Multimodel assessment of the factors driving stratospheric ozone evolution over the 21st century, J. Geophys. Res., 115, D24306, doi:10.1029/2010JD014362.

Butchart, N., et al, Chemistry-climate model simulations of 21st century stratospheric climate and circulation changes, J. Clim., 23, 5349–5374, doi: 10.1175/2010JCLI3404.1, 2010.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 18959, 2012.