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Comment

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

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Reply to Reviewer #1

The authors are grateful to the Reviewer for constructive and valuable comments that helped to improve the manuscript. Our response to each comment begins with AU-THOR and reviewer comments begin with REV.

Summary 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

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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

REV:

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.

AUTHOR:

The main purpose of our work is to investigate a slower ozone increase over Antarctica in December compared to September-November, rather than an analysis of ozone recovery in general. The original manuscript did not convey this purpose clearly enough and is now modified. At the same time, following constructive suggestions in the above comment, more previously published papers are added to the discussion, and the already referenced papers are discussed in more detail.

REV:

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?

AUTHOR:

To our knowledge, there has been no study of a relatively slow projected decadal in-

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crease in total ozone column (TOC) during the last month of the ozone hole season. The Austin et al. paper considers TOC and chlorine loads on a global scale and annual mean TOC for the months of October and March. Our study focuses on the Antarctic region during September-December, describes the decadal speeds and rates of TOC increase, and utilizes CCMVal-2 temperature and wind projections at various lower stratospheric altitudes in order to investigate possible reasons for slower TOC increase in December. Technical aspects of our analysis that differ from the Austin et al. (2010) study include using median values for deriving trends (instead of their statistical analysis method) and a bootstrapping technique for our uncertainty analysis. This discussion has now been added to the manuscript.

REF:

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?

AUTHOR:

Different from our study, Oman et al. (2010) consider TCO and Cly on a global scale, as well as their annual averages and October averages over the SH polar region. The emphasis of their paper is the spring maximum ozone depletion over polar regions and the dominant attribution of halogen levels. While Oman et al. (2010) investigate partial ozone columns above and below 20 hPa, relative to a 1960 baseline, and at the same latitude range as our study, they average over a larger vertical range than we do. We analyze ozone, temperature and winds at every pressure level between 150 hPa and 20 hPa available in the CCMVal-2 output, where the Antarctic stratospheric ozone density is at its maximum, in search for causes of relatively slow TOC increase in December. A discussion of these aspects is now added to the manuscript.

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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?

AUTHOR:

Although the above paper focuses on summer averages of temperature in response to ozone recovery and increasing GHG for both NH and SH polar regions, similar to our study, Buchart et al (2010) found that polar warming in the simulations is a direct radiative response to increasing levels of ozone. This paper is now referenced and discussed in our manuscript.

REF:

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.

AUTHOR:

Additional discussion of these documents has been added to the manuscript.

REF:

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

AUTHOR:

Several single model papers have been cited in the manuscript before, and a few more papers have now been added. These are Perlwitz et al. (2008) that use the GEOSCCM model, Akiyoshi et al. (2009) whose study considers CCSRNIES output, Hurwitz et al. (2009) that look at UKCA data, and Deushi & Shibata. (2011) that use the MRI model. All of these individual models participated in the CCMVal-2 activity.

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2. I think the choice of 1970-1979 is unfortunate, as previous studies have either used 1980 or 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.

AUTHOR:

Although the baseline choice was not important for the main purpose of our study (to analyze the slower decadal TOC increase in December compared to spring months), we agree that adopting a more widely used baseline will be beneficial. As suggested by the Reviewer, the baseline has been changed to 1965 (1960-1969), and all corresponding description and results sections have been modified accordingly.

REF:

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.

AUTHOR:

We agree that EESC is a major factor in ozone recovery and needs to be mentioned. The role of EESC has been considered in great detail in various studies, and some discussion and references to these studies are now added in our manuscript. We also note that for the purpose of our study – investigating the slower decadal ozone increase in December, temperature and winds are two important parameters. The EESC-based mechanisms that affect TOC increase are reflected in temperatures (e.g. colder temperatures lead to a slower decrease in westerly winds and thus a prolonged

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polar vortex).

REF:

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?

AUTHOR:

We agree with the Reviewer, and the section title has been changed.

REF:

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

AUTHOR:

The section title has been changed as suggested.

REF:

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

AUTHOR:

Methodology is now a separate section and the TOC description has been moved to the Results section.

REF:

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

AUTHOR:

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Section 4 has been restructured accordingly.

MINOR COMMENTS

REF:

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.

AUTHOR:

As the text has been modified and improved according to the both Reviewers' comments, this sentence has been removed.

REF:

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.

AUTHOR:

The sentence has been amended to clarify this. GEOSCCM only provided a REF-B2 simulation from 2000 onwards, therefore a baseline could not be established under the same single projections of anthropogenic forcings alone.

REF:

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)

AUTHOR:

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We agree with this comment, and the last paragraph has been removed.

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

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