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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 #2

The authors thank the Reviewer for these constructive and valuable comments that helped to improve the manuscript. Our detailed answers are given below under AUTHOR in response to reviewer comments that are marked as REF.

Summary

REF:

The authors analyse CCMVal-2 model simulations for recovery dates and other aspects

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of the future evolution of the Antarctic ozone hole. The data have been analysed for these aspects before, so the authors needed to find new angles to study this subject. They use medians not means for the multi-model analysis; this assigns less weight to outliers in the multi-model ensemble.

I have several comments on the paper. The major comment is that at times it's a little tedious to read, namely over long stretches of text it is presenting a sequence of numerical facts extracted from the data set. I suggest that some of the text could be shortened and some of the findings summarized in one or more tables. This would shorten the paper and make it more readable. Along the same lines, several sentences (explaining details of figures) in the main text should become figure captions. The claim that the analysis is more robust when using medians rather than means should be substantiated. I fail to see why using 1975 as the base year should be superior to using 1980 as the base year. In both cases, the TOC is already noticeably affected by anthropogenic ozone depletion (figures 1 and 2), but choosing 1975 makes the results incomparable with other published results on this that often refer to the 1980 baseline. If a deviation from 1980 is necessary, it would make sense to refer to the start of the simulations, i.e., 1960-1969. Finally, more reference to previous studies on the same set of data (e.g., Austin et al., 2010, and SPARC, 2010, chapter 9) are needed because the recovery of the ozone hole has been studied before in a few papers, using slightly different techniques. The paper does not adequately discuss these papers. Apart from the subtle differences in approach (bootstrapping instead of averaging) I didn't get a sense of what the fundamental differences are w.r.t. these earlier studies. On the whole, I think the paper is publishable subject to a major revision which needs to focus on what's new relative to existing literature, and how the presentation can be improved.

AUTHOR:

We appreciate these constructive comments (many of which also align with another Reviewer's suggestions) and have incorporated them into the revised manuscript. Some of the lengthy descriptions have been shortened, and others have been moved

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to the figure captions. The baseline has been modified to 1965 (1960-1969), as suggested, and the methodology, analysis, and results have been revised accordingly.

All papers suggested by both Reviewers have been referenced and discussed in the revised version of the manuscript. Also several already referenced papers have been discussed in more detail. These are described in our reply to comment #1 from Reviewer #1.

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. To our knowledge, a relatively slow projected decadal increase in total ozone column (TOC) during the last month of the ozone hole season has not been reported before. While, similar to other studies, we also discuss the ozone hole recovery in general, our main reason for providing the analysis of ozone VMR, winds and temperatures at various pressure levels in the lower stratosphere is to investigate the factors responsible for the projected slower decadal increase of ozone in December. This is now emphasized in the revised version of the manuscript.

Minor comments:

REF:

P18960L12 ff: This sentence is too complicated. How about expressing recovery as a percentage of the difference in total ozone relative to the maximum depletion (baseline – 2000 conditions). So for example 50

AUTHOR:

Following this suggestion, the proposed way of analyzing the TOC decadal increase rates has been added, and the results are reflected in the revised Table 1 (which now combines both Tables from the original manuscript).

REF:

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P18960L23: The ozone hole certainly provided a sense of urgency to the Montreal Protocol negotiations but they were already ongoing in 1985. Without the hole, people might have settled for a more toothless agreement, though.

AUTHOR:

As suggested, the sentence has been amended to state that the ozone hole discovery contributed to the establishment of the Montreal Protocol.

REF:

P18960L26: I would replace “emissions” with “abundance” here. Emissions are typically not “observed”.

AUTHOR:

The text has been amended as suggested.

REF:

P18961L1: For many species the emissions are now zero, so not “in decline” anymore.

AUTHOR:

As suggested, the text has been modified.

REF:

P18961L6: Replace “indication” with “example”

AUTHOR:

Corrected.

REF:

P18961L15: “with the recovery: : :”

AUTHOR:

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Corrected.

REF:

P18962I1: Replace “moderated” with “affected “

AUTHOR:

Corrected.

REF:

P18963I3: This is a strange formulation. In the REF-B2 simulations, only anthropogenic forcings, but not natural forcings (solar variability, volcanic aerosol), are considered.

AUTHOR:

This sentence has now been removed from the manuscript.

REF:

P18963L8: More models had an internally generated QBO. How about EMAC, UМУKCA, and UMETRAC?

AUTHOR:

EMAC and UMETRAC did not supply data for the REF-B2 simulations and thus are not relevant to our study. However referring to the SPARC report (2010), UМУKCA models do have an internally generated QBO, and our study does use the UМУCKA-UCAM model data. To reflect this, the text has been corrected and the reference has been added.

REF:

P18965L18: “the analysis presented below”

AUTHOR:

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Corrected.

REF:

P18965L19: Please name names here. Which models do you use?

AUTHOR:

The 4 models used are now specified in the text.

REF:

P18965L20: “change the main results”

AUTHOR:

Corrected.

REF:

P18965L26: “the multi-model mean”

AUTHOR:

Corrected.

REF:

P18965L26: Replace “It is also worth reminding that: : :” with “Moreover,”.

AUTHOR:

Corrected.

REF:

P18966L20: Somewhere we should have a discussion on what is in these uncertainty calculations, and what is not. Since all models use the same scenario, mainly the uncertainty is due to model formulation. Any scenario uncertainty is not included in the

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range, but could be more substantial than the model uncertainty.

AUTHOR:

We agree that while the multi-model median uncertainty calculations were undertaken in this study, the REF-B2 scenario uncertainties were not discussed. The latter were considered in detail by Charlton-Perez et al (2010). This paper is now referenced, and the details on scenario uncertainties from this study are included in the revised manuscript.

REF:

P18967L9: Salby et al didn't find that. They quoted possibly outdated literature (pre-dating CCMVal-1 and 2) on this.

AUTHOR:

As the manuscript text has been modified, this sentence and the reference to the Salby et al (2011) paper were removed.

REF:

P18967L20: Can you give uncertainty ranges on these trends? Otherwise we can't be sure whether they're actually different. Also in giving these trends, it's very important to use the same time periods for both trends.

AUTHOR:

This part has been removed from the revised manuscript, as it was not particularly relevant to our results.

REF:

P18968L2: This sentence for example should be moved into the caption. Give interpretations or scientific findings derived from figures in the main text, but explanations on how to read the figures in the captions.

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AUTHOR:

The text has been amended as suggested.

REF:

P18968L13: Do you mean ozone-poor air from the mesosphere?

AUTHOR:

This sentence has been modified to better explain our point. We note that the B-D circulation does not refer to the descent of mesosphere air. Increased B-D circulation would result in more ozone being transported from the tropical stratosphere to the Antarctic stratosphere once the polar vortex breaks down.

REF:

P18970L2: This only holds for polar-vortex air, where heterogeneous processes govern ozone depletion. Outside the polar vortex, gasphase ozone depletion is ruling the ozone abundance. It has an opposite temperature dependence, i.e., stratospheric cooling causes ozone depletion to slow down and ozone to increase. I would stick “heterogeneous” in to make it “temperature-dependent heterogeneous polar chemistry, and”. I don’t think you discussed what you mean by “ODS reactions” anywhere before.

AUTHOR:

As suggested, ODS reactions have been reworded to describe chlorine activation on PSCs.

REF:

P18972L21: I would not include any units in this formula and not call the relative change in wind speed “U”. How about $\Delta u = [u(t) - u_{base}] = [u_{peak} - u_{base}]$ and Δu can be expressed in units of percent.

AUTHOR:

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We agree with this suggestion, and the formula has been changed accordingly.

REF:

P18974L18: Are you sure about this? Eyring et al. (2010) found that climate change has to be considered for polar ozone and as a whole, the ozone layer will never recover to its pre-depleted state because of climate change. Also many people expect ozone super-recovery, i.e., more ozone over the poles in the future than before the injection of halogen.

AUTHOR:

We agree that this sentence should be clarified. Waugh et al, (2009) found that there was minimal variation in both TCO and ozone at 50 hPa, averaged over 80-90°S during October, due to climate change alone. The sentence now specifies Antarctica, to differentiate from the northern polar region as early recovery is predicted to occur at those latitudes.

REF:

P18975L7: I think you need to be clear about the role of CO₂. Increasing CO₂ probably leads to increasing global ozone but locally decreasing ozone, long-term, over the tropics and maybe over the poles (where this depends on the halogen loading). So I think your analysis of the impact of CO₂ is simplifying matters to a degree.

AUTHOR:

We agree with this suggestion, and the text has been amended to reflect this.

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

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