**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 #2

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

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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.

Minor comments:

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

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.

P18960L26: I would replace "emissions" with "adundance" here. Emissions are typically not "observed".

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

P18961L6: Replace "indication" with "example"

P18961L15: ""with the recovery…"

P18962l1: Replace "moderated" with "affected "

P18963l3: This is a strange formulation. In the REF-B2 simulations, only anthropogenic forcings, but not natural forcings (solar variability, volcanic aerosol), are considered.

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

P18965L18: "the analysis presented below"

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

P18965L20: "change the main results"

P18965L26: "the multi-model mean"

P18965L26: Replace "It is also worth reminding that…" with "Moreover,".

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

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 They quoted possibly outdated literature (predating CCMVal-1 and 2) on this.

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.

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

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

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.

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

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.

P18975L7: I think you need to be clear about the role of CO_2. Increasing CO_2 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 CO_2 is simplifying matters to a degree.

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