

Interactive comment on “Multi-model assessment of stratospheric ozone return dates and ozone recovery in CCMVal-2 models” by V. Eyring et al.

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Reply to Referee 1

We thank the reviewer for the constructive comments. The reviewer has pointed out several suggestions for improving the paper. We have considered carefully each of the comments and have modified the text accordingly. Detailed answers to the reviewer's comments are given below.

Summary This manuscript presents a multi-model comparison of the development of the ozone layer under climate change and changing ODSs between 1960 and 2100. The study uses a set of model simulations that allow the effects of climate change and ODSs on the ozone layer to be separated and introduces the difference
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between return dates (i.e., when ozone levels return to some specific value in the past), and full recovery (i.e., when there is no effect attributable to ODSs anymore), an issue that arose from new scientific insight gained in recent publications that ozone recovery is not solely a function of decreasing ODS concentrations but also dependent on climate-induced changes in stratospheric transport and temperatures. The manuscript is therefore an important contribution to the literature. It is well written and I recommend publication with some minor and technical comments the authors may wish to address.

Minor comments

P11662L20-25: This is a soup of references, you may want to split them up according to what effect they describe (chemistry, transport, or temperatures). I also miss the key papers that first made the important connection between increasing GHG and the changes in the ozone distribution through changing transport, i.e., Shepherd [2008] and Li et al. [2009, ACP]. Please add these references already here.

Done.

P11671L5: There seems to be an inconsistency in your simulation setup that may explain the discrepancy from linear additivity, expressed in largest differences between the REF and fODS+fGHG curves found around 2000 when the halogen loading is peaking. You use fixed halogens in the radiation code for the fODS simulations, but you run the fODS simulations with SSTs from an ocean run that had a specified scenario for GHGs including halogens. This may lead to an inconsistency in your energy balance and introduce some spurious effects. This should be mentioned as a caveat in your multi-model comparison.

This is true. If the radiative forcing from the transiently changing ODSs was suf-

cient to contribute to changes in SSTs, and those SSTs were used as boundary conditions for the fODS simulation, then there would be a disconnect between the prescribed SSTs in the fODS simulation and the SSTs that would have evolved under fixed 1960 ODS radiative forcing. We have now added this as a caveat in the related material in the paper. We thank the reviewer for making this observation.

P11680L13ff, P11682L27, and other places: How do you calculate total ozone column? Is it the stratospheric or the full (stratospheric+tropospheric) column? If you use the latter, you may have to check if the different treatment of tropospheric chemistry in the different models may not have caused the uncertainty in the return dates.

We indeed show the total column ozone as in the SPARC CCMVal Report, since the trends in tropospheric column ozone in the models under the SRES A1B scenario are small. This could be different for simulations with higher ozone precursor scenarios, e.g. in the RCP 8.5 scenario that is analyzed in Eyring et al., GRL, 2010.

Technical comments

P11665L16: A1b should be A1B

Done.

P11669L7: You should give the link to the supplementary material here, and not on page 11670L9.

Done.

Table 1: does upper level mean model lid height?

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It means uppermost computational level and this has been clarified.

Table 2: This table and caption are not totally clear to me, although it seems to be crucial to understand the simulations' set-up. It contains two columns named with comparison to CCMVal reference run, which are basically identical. Also, you explain REF-B2 in the caption, but this notation does not appear in the table. The information you want to convey here, I believe, is which model runs (which include both ODS and GHG effects) you use as a reference to compare with the fGHG and fODS simulations, no? So I suggest naming the first column with REF in the table header, then renaming your REFs in the table with REF-B2, since you take for most of the CCMs the CCMVal-2 simulation for comparison. For E39CA and EMAC-FUB, you may use SCN-B2d (REF-B2 including natural variability) as you explain in the text. The second column of comparison to CCMVal reference run can then be deleted. I would also use vertical lines to separate the columns with info belonging to the description of REF, fODS, and fGHG, respectively. Another point is, what does the YES mean in the fGHG ? GHG fixed at 1960 levels for radiation? You specify for most of the models CH₄, CO₂, and N₂O, for CMAM you add CFCs, but what are the SOCOL and ULAQ different from those?

We have removed the column 'Reference simulation' as suggested and made clear that the entries refer to the fixed ODS and fixed GHG simulations. We keep the second column since it includes important information in the fGHG column. The YES in the fGHG 'GHGs fixed at 1960 levels for radiation' column has been clarified.

And only out of curiosity, why do you use CCMVal-1 simulations for GEOSCCM and not CCMVal-2 simulations?

There was only a CCMVal-1 sensitivity simulation available for this model.

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The quality of most of the figures is very bad, but I assume this will be dealt with in the final version?!

The quality of the figures has been improved for the final version. Since the 8-panel figures in the submitted version were very small (see comment by Reviewer 2), we have split them into extrapolar and polar and have added the upper stratosphere in the polar regions as well. To further improve the quality of the figures, in the main paper we now only show the multi-model means while the figures that show the individual models have been moved to the supplementary material. This change significantly improves the figures.

Figure 1: Why do the 1960 baseline-adjusted ozone curves start at 5 and not 0 DU in the upper panel, to be consistent with all the following figures?

In the submitted version the baseline adjustment was made for the year 1960 in panel (a), but the 0 on the vertical axes has 0 for the 1980 value. We agree this causes confusion and have changed this now to the true 1960 baseline-adjusted timeseries, which is, by definition, 0 in 1960.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 11659, 2010.