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Atmospheric Chemistry and Physics Discussions

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

Anonymous Referee #1

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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 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.

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.

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.

Technical comments

P11665L16: A1b should be A1B

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

Table 1: does upper level mean model lid height?

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

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

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?

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