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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Contributions to stratospheric ozone changes from ozone depleting substances and greenhouse gases" *by* D. A. Plummer et al.

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We thank Reviewer Two for his helpful comments which have helped to improve the article. The reviewer's comments are reproduced below in italics, followed by our responses.

1. The authors should include a discussion of Eyring et al. Atmos. Chem. Phys. Discuss., 10, 11659-11710, 2010. Although technically still 'grey' literature, Eyring et al. has been submitted before this paper, and is publicly available. Also Eyring et al. use the same CMAM simulations discussed here. Some of the results presented here

have already been shown by Eyring et al.; where this is the case, the authors should say so. Correspondingly, there is scope for shortening this paper.

We will point out that our paper was submitted two days (!) before, and appeared in ACPD approximately two weeks before, the Eyring et al. ACPD paper. Technicalities aside, we were certainly well aware of Eyring et al., as many of us are co-authors on this paper. Yes, many of the broad, qualitative behaviours described here are consistent with the findings presented in Eyring et al. (2010). The decrease in ozone found in the lower tropical stratosphere due to the acceleration of the B-D circulation is one example of qualitatively similar behaviour described in both papers, but this finding is not new and has previously appeared in the literature. Where we discuss it here, we make reference to the earlier work. Similarly the impact of CO_2 cooling on ozone in the upper stratosphere has appeared in the literature, is referenced here, and is also discussed in the Eyring et al. paper. To address the reviewer's concern, we have added a note that the CMAM simulations described here have been submitted to CCMVal-2 and appear as part of the multi-model ensemble analyzed in Eyring et al. (2010). We have also added a few references to the text where the findings shown in Eyring et al. (2010) are most applicable to our study and add considerably to the body of previously published work.

We do not agree with the suggestion to shorten the paper where there is overlap. The analysis of the scenario runs presented here is quite different from that presented in Eyring et al.; examples include the quantitative analysis of CO_2 and ODS contributions to ozone and temperature trends, the analysis of the regression of O_3 on to EESC, or the analysis of the vertical distribution of changes in ozone column. The most glaring examples of overlap are the qualitative description of the general behaviour of the different simulations and for completeness we feel that the general discussion of the behaviour of the runs should remain part of the paper to support, and make meaningful, the more in-depth analysis.

2. I applaud the authors for using an interactive ocean in their model. CMAM is the only CCMVal model to incorporate this. An interactive ocean complicates the initialization of the model, and I have some concerns about the way this is done here. The authors report that in the first set of coupled runs, a tropospheric temperature bias was identified. Consequently, some model retuning was performed, and an ocean initial state from the year 2000 was used to restart the retuned model in 1950. Firstly, I would like to know in some more detail which retuning was performed, and secondly I wonder why the authors did not rerun the retuned CCCma parent model again up to 1950 to come up with an internally consistent initial state. The authors state that there are no drifts associated with spin-up in the model; hence this may not be a big issue.

Following a standard procedure, the model used for the first set of coupled experiments was tuned in AGCM mode with observed SSTs and sea ice so that it had roughly a zero net energy balance at the surface. Since the coupled model drifted slightly cold, a second tuning exercise was undertaken. Cloud optical properties were adjusted to produce a small positive energy balance at the surface to counteract this drift.

As the reviewer points out, the preference would have been to redo the spin-up to 1950 with this adjusted version of the model. However, as noted in the paper, the run from 1850 to 1950 is made with the 'dynamical CMAM' – a version of the model with the same set of physical parameterizations and setup as the standard CMAM, though without chemistry and using a specified ozone climatology for radiation. The dynamical CMAM is thus considerably more computational expensive than the standard CCCma CGCM and, due to the cost of the model, there remained insufficient time to perform the full initialization and meet the deadline for the CCMVal-2 data request. The decision taken was to employ the 2000 ocean state as discussed in section 2.1 and use *a posteriori* checks to ensure that this did not adversely affect the model response. Based on these checks we concluded that the initialization had little or no bearing on the model response.

3. The ocean is arguably the most interesting and distinguishing characteristic of the CMAM model. However, not much is made of this aspect of the model. For the most part, I would expect results very similar to those presented here coming out of a version of CMAM without interactive ocean (as has been shown by Eyring et al., who presented results from a group of models almost exclusively without interactive ocean). I encourage the authors to present, in a follow-up paper, results that show the benefits of running with an interactive ocean.

We agree with the reviewer that this is an essential follow-up study and an analysis of the ocean response is currently underway.

4. The authors force the simulations using the A1 and A1b scenarios. They show that faster overturning, caused by climate change, decreases the lifetime of N2O considerably (by about a quarter or so). I wonder whether this effect has been accounted for in the definition of the A1b scenario. It might mean that a large portion of the projected increase in N2O emissions (due to intensifying agriculture and other reasons) may be counterbalanced by an acceleration of chemical loss. The model does not account for this as it is forced with surface abundances of N2O not emissions. The same reasoning also applies to the halocarbons. This question is slightly outside the scope of the paper but the authors may wish to address it briefly.

This is a very interesting effect that impacts the current generation of CCMs, somewhat analogous to the situation of CGCMs with some simulating the carbon cycle and others using prescribed CO_2 concentrations. We are not certain to what extent any acceleration of the B-D circulation has been taken into account when the concentration-driven scenarios for N_2O and the halocarbons are developed with the integrated assessment models. In any case, the specification of the lower boundary condition as a concentration, as opposed to a flux, artificially constrains the CCMs. As we discuss the effects of the increased B-D circulation on N_2O, we have added a brief comment mentioning the artificial constraints imposed by the use of a specified concentration at the lower

boundary.

5. The CMAM model exhibits a striking difference in ozone recovery between the northern and southern hemispheres, with Antarctic springtime ozone being largely insensitive to climate change, whereas Arctic springtime ozone recovers more quickly due to climate change. Is this result corroborated by the results from other CCMs as discussed by Eyring et al., or other CCMVal-2 papers?

The Eyring et al. article clearly shows a similar effect. The Eyring et al. article presents the recovery date of total column ozone and EESC at 50 hPa derived from the multi-model ensemble. The date of recovery, to 1980 values, for Arctic springtime ozone (2027) is considerably earlier than for Antarctic springtime ozone (2052). The ensemble mean results are qualitatively similar to the CMAM results, though CMAM shows both an earlier recovery to 1980 values for total column ozone over the Arctic (2015) and a later recovery over the Antarctic (2070). Our discussion of polar ozone focuses on the effects of ODSs given by the difference between the GHG and REF-B2 experiments and not on the return dates of total column ozone in the REF-B2 experiment. Therefore we do not present any comparison with the CCMVal-2 multi-model results in the text of the article.

Minor comments:

P 9649, l2: I had to read this sentence three times before understanding the structure. Please rephrase.

It has been done. We hope the sentence is now easier to read.

P 9650, l 7: Waugh, Nature Geosci, 2, 14-16, 2009, maintains that observational evi-

dence so far does not imply a speed-up of the BDC. Maybe you can include reference to this paper here.

The Nature Geoscience article by Darryn Waugh is an unrefereed News & Views piece prefacing a letter by Engel et al. (Age of stratospheric air unchanged within uncertainties over the past 30 years, 2, 28-31, 2009) that reports on the age of stratospheric air inferred from trace gas measurements over the northern hemisphere mid-latitudes. The discrepancy between the models and observations for age of air is certainly provocative, but as the Waugh article points out there is some observational evidence for an increase in upwelling in the tropical lower stratosphere as predicted by the CCMs. As discussed by Engel et al. (2009), one possible way to reconcile an increase in tropical upwelling with observations of little change in mid-latitude age of air would be through changes in poleward transport of air in the lowermost stratosphere. To make reference to this issue, without delving too deeply, we have added the following sentence:

However, it should be noted that there may be some discrepancy between model predictions of changes in the stratospheric circulation and estimated changes in age of air over the recent past inferred from the available observations (Engel et al., 2009).

P. 9651, L22: I wonder why this is so. All other stratospheric CCMs incorporate a representation of NAT formation. While polar heterogeneous chemistry is one reason for large inter-model differences in the CCMVal group, I think omitting NAT is clearly not the way forward.

We do not mean to imply that the way to advance CCM modelling in general is to omit a representation of NAT. This particular sentence is merely intended to make clear that the model lacks this capability, which could affect the results. The main reason we have held back on including NAT in CMAM is the current uncertainty over the importance of NAT to halogen activation and the uncertainty over how to parameterize NAT in models

(particularly CCMs that cannot afford an extensive and expensive parameterization), as we discussed on p.9658 of the submitted version of the paper. We have had discussions within the group on how best to include a representation of NAT, but from a pragmatic point of view it is not clear how this would improve the CMAM simulation of ozone depletion.

P. 9653 L17: Please clarify whether the 'long-lived GHGs' include the CFC-11 and CFC-12 species here.

We have added an explicit mention of CFC-11 and CFC-12 here.

P. 9656 L 5: Replace 'comes to' with 'undergoes'.

We much prefer 'comes to a minimum'. The phrase 'undergoes a minimum' has connotations of something being forced externally, while here we are describing the vertical variation of the effect of ODSs which displays a minimum 'around 10 to 20 hPa'.

P. 9669 R1: This is likely the most temperature sensitive bimolecular reaction in the CMAM model (judging from the Arrhenius coefficient). Please confirm.

A review of the rates of the bimolecular reactions in the CMAM mechanism does show the reaction N + O_2 to be the most temperature sensitive, with an activation temperature (E/R) of 3600K. The reaction of $O(^3P)$ + HCl is not too far behind, however, with an activation temperature of 3300K.

Figure 10: I think there is too much information in the figure. Please omit or simplify.

We admit Figure 10 is a bit dense, but the presentation of vertical changes in ozone is always very difficult. To present the changes in mixing ratio makes relating the changes

to column amounts very difficult. Another commonly used method is to present the changes in units of DU/km (a linear scaling of number density), however the area under the curves must be 'visually integrated' to arrive at a change in the total column of ozone.

For Figure 10 we present the cumulative vertical integral from 0.1 hPa down to a particular level. We feel this very nicely presents the vertical distribution of changes in the ozone column since one can see both local changes and cumulative changes. The zero on the x-axis is in reference to the 1960-1974 average column, and positive values mean that the vertical column above that point is larger than the 1960-74 average. The slope of the line then gives the local difference in ozone relative to the 1960-74 average. The current form of the figure also makes analysis of changes in the partial column of ozone over broad vertical slices easy to read off. The text (including the Abstract) makes frequent reference to the changes in the vertical column of ozone above 20 hPa, and that information can be directly gleaned from Figure 10. While it does take a little effort to decipher, we feel that the current presentation of Figure 10 best illustrates the changes we wish to discuss in section 3.5 (now section 3.6).

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 9647, 2010.