**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.

D. A. Plummer et al.

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We thank John Austin (Reviewer One) for his helpful comments, which have helped to improve the article. His comments are reproduced below in italics, followed by our responses.

p.9648. The abstract needs to be written to convey the information more precisely. I believe a complete rewrite would be useful, as most sentences have a problem. The abstract has the appearance of classical science paper writing: it looks as if it has been written last, and as a result has probably had fewer iterations before submission.

C6112

I favour writing the abstract first as this means that it gets the most iterations before submission.

While the reviewer is correct in his supposition that the abstract was written last, we do not believe it suffered for lack of iterations as no other section of the article was more revisited and reworked. Regardless of writing method, we have attempted to address the reviewers concerns by simplifying some of the text and reducing the amount of detail in the abstract, to convey the general ideas more precisely and render the abstract more readable.

l.2-5 'experiment' is usually used to describe the model set up rather than the outcome. I suggest 'experiments' → 'simulations'. Even then, the question arises as to simulations of what. The sentence is probably trying to convey too much information and perhaps should be split into two.

We have used the term 'experiment' to denote the results from one particular model scenario, as estimated by an ensemble average of three simulations. We feel that the term 'experiment' more correctly, and compactly, conveys the sense that the results are representative of the scenario, as calculated from the ensemble of simulations, than from a particular simulation. While we choose to keep the term 'experiment', the sentence has been shortened.

l.5 is confusing. 'evolution of ozone' breaks up the causes (GHGs) on the effects – B-D circulation increase and cooling.

The sentence has been made more general to eliminate specific reference to the B-D circulation increase and cooling.

l.8 This suggests that GHG effects and ODS effects can be unambiguously separated.

C6113

It is probably clear enough in the upper stratosphere, but in the lower stratosphere, the method by which the effects are 'separated' needs to be specified.

We have modified the wording of the passage, from 'Separating the effects...' to 'We estimate...', to make the statement less definitive. A discussion of how the effects have been separated can be found in section 2.1 of the article. Such a topic is difficult to adequately address in an abstract.

l.10 It is unusual (and a bit confusing) to specify the ozone ODS effect as a fraction of the net ozone change. It would be more usual to indicate that the net ozone change is smaller than the pure chemical effect by x% rather than the way phrased.

The text now states that 'the net decrease in the column amount of ozone above 20 hPa is approximately 75% of the decrease that can be attributed to ODSs'.

l.11 et seq. Doesn't this just say the same as the previous sentence but in more conventional language?

We believe the comment relates to the sentence on line 8 beginning 'Separating the effects of GHGs and ODSs...' and the sentence directly following that beginning on line 11 as 'Over the 21st century, as ODSs decrease, continued cooling from CO_2...' We point out that the first sentence presents the contribution of ODSs and CO_2-cooling to ozone changes for the period up to 2000 when ODS concentrations were rapidly increasing. The second sentence presents a similar attribution, albeit for the 21st century when ODSs are decreasing.

l.13 What happens between the 'upper stratosphere' (pressure not specified) and 20 hPa?

The text has been clarified to say that the discussion of changes in the 'upper strato-

sphere' was meant to apply down to 20 hPa.

l.13 This is a sentence trying to convey too many ideas. The global average is relatively unaffected by the B-D circulation change (what goes up in one place comes down in another). Are you trying to say that the increase in the B-D circulation contributes to a change in ozone patterns? If yes, then say so more directly, possibly using more sentences.

The sentence has been broken up to more precisely convey the different ideas. The most apparent effect of the increased B-D circulation is the decreases in ozone in the tropical lower stratosphere and an increase in the extra-tropics and this is now simply stated. We also want to put forward the finding that the changes below 20 hPa, when averaged over the globe, do not sum to zero. We have added the sentence:

In addition to a latitudinal redistribution of ozone, we find that the globally averaged column amount of ozone below 20 hPa decreases over the 21st century, which significantly mitigates the effect of upper stratospheric cooling on total column ozone.

l.16 This sentence is misleading, because this result really depends on the model setup. The B-D circulation increase will speed up the removal of CFCs from the atmosphere (Butchart and Scaife, 2001) and speed up recovery. However, in a model in which CFC mixing ratios are SPECIFIED in the troposphere this doesn't work: an increase in circulation rate increases the flux of CFCs and could delay the recovery of ozone. The details would depend on the specification of the tropospheric CFCs.

The reviewer's comment is quite correct. Our intention was that the statement 'changes associated with GHGs do not appreciably alter the recovery of stratospheric ozone from the effects of ODSs' be interpreted in a narrow sense to mean ozone changes that can be attributed to ODSs (from the difference REF-B2 - GHG) follow the time evolution of reactive chlorine and bromine throughout the length of the simulation. In other words,

that GHG-related changes do not alter the relationship between the estimated effects of ODSs on ozone and the local concentration of reactive chlorine and bromine. The passage has been altered to avoid any broader interpretation. The passage now reads:

Analysis by linear regression shows that the recovery of ozone from the effects of ODSs generally follows the decline in reactive chlorine and bromine levels...

l.17 What ozone recovery definition are you using? The WMO one only defines full ozone recovery from ODSs. It seems that recovery is being used to mean ozone increase in general (from whatever causes).

This passage has been modified to specify 'recovery of ozone from the effects of ODSs' since we are speaking here of the estimated ODS effect on ozone derived by the difference between the REF-B2 experiment and the experiment with ODSs held constant at 1960 values.

l.21-27. This contains duplicated material. The CO2 cooling effect in mitigating NOy increase is mentioned in both sentences.

The first instance of the mention of CO_2 cooling has been removed.

p. 9648 l.2-4. This is a clumsy opening sentence. It has a certain Latin ring about it: the relevant verb is almost at the end of the sentence!

The reviewer is referring to p.9649, l. 2-4. The opening sentence has been reorganized and shortened to make it more easily read – and perhaps with a more modern ring to it.

l.17 'chemical rates' → 'chemical reaction rates'

Done

p. 9650 l.25 Linear regression quantifies relationships: it cannot apportion cause and effect, which requires physics.

Given that multiple linear regression (MLR) is based on a purely statistical (non-physical) relationship between time series, we certainly do not mean to imply otherwise here by referring to 'cause and effect'. In fact, we do not state that MLR could be used to 'derive cause and effect', for which physics is clearly required. Rather we state that MLR could be used to 'apportion cause and effect', with the implication that MLR is used to estimate the contribution of different causes (predictors) to the behaviour of an effect (predictand). We don't believe that the statement 'apportion cause and effect' implies relationships between variables derived by MLR are on a solid physical basis any more than when MLR is used to estimate the contribution of, say, trends in CO_2 and ozone to stratospheric cooling.

l.29 Rather it's the other way around: until relatively recently 3-D models were inhibited by their large computational cost. Or is the point that you are making that 'similar scenario' simulations somehow save computer time? I'm afraid I'm lost, as I'm not familiar with the phrase 'similar scenario' and if it's that important it should be explained.

The sentence has been restructured for clarity.

p. 9651 l.24 'Model' is already in the acronym so CMAM model is a bit of a howler. Please check for this problem in the whole document.

Admittedly, to expand out the acronym one ends up with a rather nonsensical phrase when referring to the 'CMAM model'. This was the only instance where 'CMAM model' has been used and it has been changed.

p.9652 l.10 There is of course a whole group of articles in press for JGR and you may or may not wish to refer to the CCMVal report itself.

The reference has been modified to explicitly refer to the CCMVal report in addition to the JGR issue.

p. 9655 l.2 More precision in the English is needed. In a paper on ozone change, this isn't the 'general behaviour' of the different experiments at all. It is the temperature trend: no more, no less.

The text has been modified to more directly introduce the first figure.

p. 9658 l.21-26 Of course the CCMVal Chapter 9 material has been submitted for publication in JGR and I hope will be accepted soon. But what exactly are you trying to say here? I think it is a weak argument to suggest that the observations are more variable in their dynamics than the models. I'm not sure if the apparent poor Arctic performance of models lends credence to anything, especially bearing in mind the uncertainties in the PSC schemes already discussed.

The section discusses possible reasons for the underestimation by CMAM of negative trends in ozone in the lower stratosphere at extratropical northern latitudes. Observations show decreases in ozone of between -4 and -8% over the 1979-2005 period, while CMAM shows trends no larger than -2% and positive trends below 150 hPa. Trends in ozone over this region are of importance because they cover heavily populated regions of the northern hemisphere. The discrepancy with observations is significant and, judging from the trends in total column ozone from the CCMVal models, CMAM is not the only CCM that has difficulty representing trends in this region. For these reasons we discuss the discrepancy in trends and present some of the difficulties in representing PSCs, particularly NAT, in CCMs given our current understanding. The reference to dynamical variability is a reference to the decadal-scale (or at least multi-

year) variations in the dynamical nature of the lower extra-tropical stratosphere that may produce large apparent trends. An example is the series of very cold winters in the Arctic stratosphere in the mid-1990s that had impacts on transport of ozone to high latitudes and the amount of chemical destruction of ozone in the Arctic vortex. Since the processes that give rise to these multi-year variations are poorly understood, and may very well arise from the internal variability of the coupled atmosphere-ocean system, models will not reproduce these. The text has been modified to more explicitly identify 'decadal-scale' variability and the effect on 'apparent trends'.

l.29 Poorly phrased: the way that it is written one might conclude that cooling produces ozone depletion.

The passage has been modified to more clearly state that the decrease in ozone in the REF-B2 simulation results from the effects of increased halogens, partially offset by cooling.

p.9659 l.2-4 This isn't at all clear from figure 2, particularly as the largest effects on ozone occur near 2 hPa, and there is some latitudinal variation. Also, see comment on the abstract, l.10.

The derivation of the contribution of ODSs to the change in ozone in REF-B2 requires dividing the ODS-attributed changes shown in panel c of Figure 2 with the net changes shown in panel a – admittedly not an easy quantity to derive from the figures provided. The text in the article was based on a figure that we have not included in the article. We still choose to omit the direct presentation of the fractional contribution as we feel it does not add a great deal of information. We have added a short bit of text to say that the relative contribution is not shown and have also, following the comment on line 10 of the abstract, presented the net change in ozone as a fraction of the ODS attributed change.

l.8-9 Some quantitative comparison would be better than this vague statement. How reliable is doing the calculation with separate simulations compared with attribution using linear regression?

An initial comparison of the ODS contribution to ozone changes derived here with an estimate derived through multiple linear regression from the earlier work by Jonsson et al. (2009) showed, as stated in the paper, a 'rough accord' of the two estimates. There are differences between the two estimates and we are in the process of comparing the two methods in more detail to understand if the differences are significant and where these differences are arising. We hope to be able to provide a much more quantitative comparison of the two methods in the near future. Here we can only state that the two estimates are comparable.

p.9661 l.25 It ought to be borne in mind that we don't have reliable observed estimates of the rate of change of tropical upwelling, so none of the CCMs could be correct.

That is quite correct. The intent of the discussion was only to put into context the magnitude of the ozone trends in the lower stratosphere from CMAM relative to other CCMs. On that point, we have added a reference to section 9.3.5 of the SPARC CCM-Val report (2010) that provides a comparison of CCM estimates of trends in upwelling and trends in ozone.

p.9662. l.1-2. Have there been corresponding changes in the age of air? Convection changes might be a contributing factor.

We do find that the age of air in the stratosphere is lower in the CCMVal-2 REF-2 simulations. For the 1990s the CCMVal-2 simulations show an age of air that is typically 0.2 to 0.3 years younger than was found in the CCMVal-1 set of runs. By the 2070s the difference in age has increased to 0.3 – 0.4 years, with the CCMVal-2 runs being as much as 0.5 years younger in the upper stratosphere. Both sets of CCMVal runs show

a decrease in the age of air with time, but the decrease in the CCMVal-2 runs is more pronounced. We have not included mention of the corresponding changes in age of air in the article because we feel the discussion of upwelling mass flux is sufficient to illustrate the circulation changes relevant to the behaviour of ozone in the lower tropical stratosphere.

We can certainly not rule out changes in convection contributing to differences in the speed and acceleration of the B-D circulation, though we note that the underlying GCM has changed very little between CCMVal-1 and CCMVal-2.

L25-26. This is peculiarly worded. The changes in ozone ARE the recovery due to halogen increases.

The sentence has been shortened to remove the redundant use of 'recovery'.

p.9663. l.2. ODSs have increased by 50% in October-November? Reword.

The passage has been reworded to indicate that the changes in the annual mean ozone are driven by changes in ozone of more than 50% in October-November.

l.9. 'evolution' implies time or you could write 'temporal variation'.

Changed.

l.15 I'm not really a fan of EESC, I prefer the much more straightforward Cly. Bry and Cly are highly correlated anyway. The problem is that this expression in really only valid near 50 hPa. In the upper stratosphere (5 hPa) Bry has a much smaller effect on ozone.

There is not a single correct expression for EESC that can be used throughout the

stratosphere. We note that using Cl_y, though more straightforward, is not any more correct than including a contribution from Br_y to EESC. To assess how the definition of EESC may have affected the regression analysis, we have repeated the regression using different expressions for EESC. For EESC defined as

$$EESC = [Cl_y] + \alpha [Br_y] \tag{1}$$

we have performed the regression analysis with values of α of 5 and 60 and we find the results are very similar. As suggested by the reviewer, the timeseries of Cl_y and Br_y are highly correlated and while the value of α has a significant effect on the exact value of EESC, the relative variation of the EESC timeseries is little affected by the definition of EESC. What we mean by 'relative variation' is more clearly illustrated in Figure 1. Panel a shows the value of EESC derived from the WMO A1b scenario using Cl_y and Br_y calculated using the method of Newman et al. (2006) for an age of air of three years and values of α of zero and 60. As can be seen in panel a, the value of α has a significant effect on the absolute value of EESC. However, regression will fit the EESC timeseries to the ozone timeseries by scaling the EESC timeseries up or down. Therefore, it will be the relative change of the EESC timeseries as opposed to the absolute value that is important for regression. Panel b of Figure 1 shows the same two EESC timeseries, though with the value at 1960 set to zero and the peak value normalized to a value of one. The normalized timeseries for the two values of α are very similar, which explains why the regression of ozone on to EESC is relatively insensitive to the value of α that is used.

p.9667-9670. Subsection 3.4 is already quite long and I particularly liked the description of the N2O/NOy changes. So I would suggest putting this material into a separate subsection.

Quite happy to follow this suggestion.

<div align="center">C6122</div>

p.9672. l.27-29 The sentence is a bit clumsy and needs to be rewritten more clearly. It would not be a sign of weakness to use two sentences.

The sentence has been reworded slightly and shortened to remove the hanging phrase 'to the changes in ozone column.'

p.9670-9675. Subsection 3.5 is very long and unevenly structured. I would suggest turning into 3.5, 3.6 and 3.7 based on the individual figures 9-11 respectively. [Or 3.6-3.8 if you take my earlier advice on 3.4]

The significant change in discussion between Figures 9 and 10 has now been made more clear by adding a subsection for the discussion of the vertical changes in ozone (Figure 10). The discussion of polar ozone has been promoted one level to a proper subsection.

References

Newman, P. A., E. R. Nash, S. R. Kawa, S. A. Montzka, and S. M. Schauffler, When will the Antarctic ozone hole recover?, Geophys. Res. Lett., 33, L12814, doi:10.1029/2005GL025232, 2006

Fig. 1. Panel a shows the absolute value of EESC derived for the WMO A1b scenario for a mean age of air of three years using values of α of zero (green line) and 60 (black line). Panel b shows the same two timeseries, though normalized to have a value of zero at 1960 and a peak value of one. **The figure is available in the Supplement.**

C6124