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Comment

***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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Received and published: 11 May 2010

Review of the paper ‘Contributions to stratospheric ozone changes from ozone depleting substances and greenhouse gases’, by D. Plummer et al., submitted to ACPD.

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The manuscript uses a sequence of coupled chemistry climate model simulations for a range of external forcings to determine the separate influences of dynamics and chemistry on stratospheric ozone amounts. The simulations and their analysis are an important contribution to the understanding of ozone in the atmosphere. However, while I understand generally what the authors are saying, the English is a bit contorted

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in places. The abstract in particular needs much more work.

Overall, my comments are minor and a revised manuscript will be very well suited for publication in ACP.

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

I.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.

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

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

I.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.

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

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

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I.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.

I.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.

I.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).

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

p 9648 I.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!

I.17 'chemical rates' → 'chemical reaction rates'.

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

I.29. Rather its 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.

p.9651 I.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.

p.9652 I.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.

p.9655 I.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.

p.9658 I.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.

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

p.9659 I.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, I.10.

I.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?

p.9661 I.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.

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

I.25-26. This is peculiarly worded. The changes in ozone ARE the recovery due to

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halogen increases.

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

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

I.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 is really only valid near 50 hPa. In the upper stratosphere (5 hPa) Bry has a much smaller effect on ozone.

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

p.9672. I.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.

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]

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

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