Atmos. Chem. Phys. Discuss., 10, C3429–C3432, 2010
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Atmospheric Chemistry and Physics Discussions

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Interactive Comment

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

Anonymous Referee #2

Received and published: 1 June 2010

Summary: The paper addresses the influences of climate change and of ozone depleting substances on stratospheric ozone, using a coupled atmosphere-ocean CCM. The authors perform ensembles of simulations, varying, in three set of simulations, only the greenhouse gases, only the ozone depleting substances, or both simultaneously. The topic is interesting and timely, and the model well-established but with an innovative edge. The results are presented in great detail; a bit of shortening would benefit the paper.

My reservations about the paper are listed below:

1. The authors should include a discussion of Eyring et al., Atmos. Chem. Phys.

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Interactive Discussion

Discussion Paper

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 as 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.

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 a 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.

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.

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 N_2O considerably (by about a quarter or so). I wonder whether this effect has been

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accounted for in the definition of the A1b scenario. It might mean that a large portion of the projected increase in N_2O 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 N_2O 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.

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 much 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?

I recommend publication of the paper after the above concerns, and the minor comments listed below, have been addressed.

Minor comments:

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

P 9650, l 7: Waugh, Nature Geosci, 2, 14-16, 2009, maintains that observational evidence so far does not imply a speed-up of the BDC. Maybe you can include reference to this paper here.

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.

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P. 9653, L5: Please list the other published papers that use your “REF-B2” simulation, or the CHM or GHG simulations.

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

P 9656 L 5: Replace “comes to” with “undergoes”.

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

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

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