Atmos. Chem. Phys. Discuss., 8, S9810–S9813, 2008
www.atmos-chem-phys-discuss.net/8/S9810/2008/

Atmospheric Chemistry and Physics Discussions

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8, S9810–S9813, 2008

Interactive Comment

Interactive comment on "What would have happened to the ozone layer if chlorofluorocarbons (CFCs) had not been regulated?" *by* P. A. Newman et al.

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Received and published: 12 December 2008

This study provides the modeling work that is required to understand the effectiveness of negotiated limits on ozone depleting substances, carried out with a state-of-the-art chemistry-climate model. The analysis is clear and thoughtful, the paper is well written, and the figures are excellent. Overall this paper is one of the best I've reviewed in recent years, and should certainly be published pending minor revisions suggested below.

Specific comments:

P20567, L8: In addition to the work that led up to the WMO 1985 report and that assessment, I believe that the discovery of the Antarctic ozone hole played a substantial role in motivating the Montreal Protocol. So I'd suggest changing "In response, the landmark Montreal Protocol"; to "In response to that assessment and the discovery of the Antarctic ozone hole (Farman et al., 1985), the landmark Montreal Protocol"

P20568, L27: Delete the word "better" at the end of this line, as 3D models don't account for longitudinal variations better than 2D, rather 2D don't account for them at all.

P20570, L23-24: The model description states only that processes involving PSC use the parameterization of Considine et al. The reader should be given a brief description here, as changes in ozone involving PSC-chemistry in the tropics are an important conclusion of this paper. The reader should not have to read the Considine paper to get a basic idea of what's included in the parameterization and how sensitivity to temperature, water vapor, etc is represented.

P20571, L5: The particular A1B scenario used should be stated here. I'd guess this is the so-called "marker" AIM model A1B scenario, but as A1B is a family of scenarios it would be best to state this clearly.

P20571, L13: It would be useful to state if the model has an internally generated QBO signal.

P20573, L2-3: Not only do the ODSs cause radiative forcing, but ozone changes themselves do, which should be added here.

P20573, L13-15: The 5-day relaxation time scale was given earlier and does not need to be repeated here.

P20575, L8-10: The 2D World Avoided run was presented in section 2, and does not need to be introduced again here.

P20578, L17: I suggest adding a statement along the lines of this: The contribution of stratospheric ozone loss to the troposphere is likely underestimated, at least season-

ACPD

8, S9810–S9813, 2008

ally, since ozone's tropospheric lifetime can be much longer than 5 days during polar night, for example.

P20579-80, Section 5, comment 1: The chemistry section needs to include a discussion of the time evolution of stratospheric water vapor in the model. Ideally, this should be separated into the contribution from methane oxidation and not from methane oxidation, and then the impacts on temperature and ozone discussed (and potentially compared with observed trends such as Randel et al, JGR, 05 and model studies such as Shindell, GRL, 01).

P20579-80, Section 5, comment 2: I believe it would also be interesting to include a discussion in this section on changes in photolysis due to the very large ozone changes simulated in these runs. Specifically, is there a change in long-lived species in the lowermost tropical stratosphere as more high-energy radiation penetrates further down? Do you see greater loss via reaction with O(1D) or OH, or greater photolysis in the window region, and how do any chemical changes in this region compare with changes due to the increased upwelling?

P20579-80, Section 5, comment 3: The finding of tropical heterogeneous chemistry is quite interesting. But observed temperatures near the tropical tropopause are already less than 200 K (e.g. in the CIRA reference dataset, they are around 195 K at the equator during January). The model results are shown at a slightly higher altitude (Figure 7), leading me to wonder: Does the model match tropical temperatures in the lowermost stratosphere? Is the model colder at lower altitudes? Does heterogeneous ozone chemistry appear at different times depending on altitude?

P20580, L20-end of paragraph: This discussion is interesting, but I find the conclusion that the 2D and 3D models give similar results very surprising. I would think that the degree of enhanced tropical lifting would vary among models (as in the intercomparison of Butchart et al, Clim. Dyn., 2006), presumably due to factors such as the convection scheme in the model. It may be that it's mostly coincidence that the two models used

ACPD

8, S9810–S9813, 2008

Interactive Comment

here happen to agree so closely. If this is really driven by tropical upwelling enhancement, as is argued here, then I suggest noting that this result may not be quite as robust as these 2 models suggest.

P20581, L18: The permanent winter in the SH is another interesting finding. It would be useful to discuss the NH summer as well. Do you see a permanent winter in the NH too, or are the two hemispheres behaving differently? (In the summary, P20584, L14, the NH summer is again not discussed, but should be).

P20581, L25: The word "a" is needed before "near linear".

P20582, L15: The word "situation" would be better than "problem" here (it's a value judgment as to what's a problem).

P20583, L28: It'd be useful to add the clear-sky tropical land area UV index values here for comparison with numbers given elsewhere.

P20592, Table 1: It would be clearer to call the heading for SSTs "prescribed" rather than "fixed" as these are not fixed in time.

Figures: Really very nicely done. Some include a great deal of information, so could have become very confusing, so I commend the authors for the care they've shown in putting these together.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 20565, 2008.

ACPD

8, S9810–S9813, 2008

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