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Atmospheric Chemistry and Physics Discussions

ACPD

8, S11659–S11667, 2009

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.

P. A. Newman et al.

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We wish to thank Dr. Shindell for his thoughtful comments. In this response, his comment is indented, and our response immediately follows.

> *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"*

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Interactive Discussion

Discussion Paper

The effect of the ozone hole's discovery provided interesting background to the Montreal Protocol. Negotiations to constrain ozone-depleting substances (ODSs) were already moving forward prior to Farman et al. (1985). The UNEP Governing Council held an initial meeting in Washington in 1977 to set up a research plan of action, followed in 1981 by the setting up of an ad-hoc working group that met between 1982 and 1985 to set up a framework agreement for protecting the ozone layer (Benedick, 1991). The Vienna Convention for the Protection of the Ozone Layer was negotiated in March 1985 (again, prior to Farman et al., 1985). Under Article 2, it states, "The Parties shall take appropriate measures in accordance with the provisions of this Convention and of those protocols in force to which they are party to protect human health and the environment against adverse effects resulting or likely to result from human activities which modify or are likely to modify the ozone layer." The NASA and WMO assessments that preceded Farman et al. (1985) were comprehensive and thorough in their outlook of severe ozone depletion in the absence of any actions.

The severe depletion of ozone over Antarctica undoubtedly strengthened the scientific basis of the human cause, but only subsequent to the Montreal Protocol signing in September 1987. The observations of ClO and O_3 using the NASA ER-2 established that the ozone hole resulted from human-produced compounds, but this was published in Anderson et al. (1989). Richard Benedick's book, "Ozone Diplomacy" discusses the impact of the ozone hole on negotiations. "During July and August of 1987 some U.S. environmentalists debated whether to put pressure on UNEP to postpone the final protocol negotiating session until after the Antarctic expedition, in hopes that its results might influence governments to agree to more stringent CFC controls. However the environmentalists worried that new data might prove inconclusive or again, as in 1986, show an ozone improvement. Such a result would undoubtedly have broken the momentum toward consensus, and the treaty negotiations would have become stalled in new bickering over the significance of Antarctica. The environmentalist finally decided that it was more prudent to take the 'bird in hand' in September 1987 rather than risk further delay."

ACPD

8, S11659–S11667, 2009

Interactive Comment

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Interactive Discussion

Discussion Paper

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.

Sentence changed. Longitudinal effects are parameterized in terms of eddy diffusion coefficients in 2-D models.

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.

Paragraph added.

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.

The A1b CO_2 scenario is taken from the CCMVal recommendation. In particular, the CO_2 is from Table II.2.1 (ISAM reference model) in Houghton et al. (2001). Appropriate text added.

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

ACPD

8, S11659–S11667, 2009

The model does not include either an internally forced QBO or a prescribed QBO. Appropriate text added.

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

Agreed. Appropriate text added.

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

Deleted.

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

Changed.

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

Added.

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.

ACPD

8, S11659–S11667, 2009

Interactive Comment

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).

The water vapor trends in GEOS-4 have been discussed in Oman et al. (2008). The total hydrogen trends in our *WORLD AVOIDED* simulation generally follow the trends discussed in Oman et al. (2008) up to the 2050s. The cooling temperatures in the tropics also leads to irreversible removal of water in the tropics, causing the water vapor in the stratosphere to drop by about 1 ppm between 2050 and 2065. This is the major change of water vapor. We have added a paragraph at the end of section 5.

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?

There are definitely changes in the lower tropical stratosphere as more UV radiation penetrates to this region with increased ozone loss. This effect competes with changes due to increased upwelling. We have made plots of the relative concentration of tropical CFC-11 to tropospheric CFC-11 between about 100 and 10 hPa. This fraction decreases from 1.0 near the tropopause to near zero at 10 hPa. The CFC-11 relative concentration at any given level increases slowly up to about the year 2020, indicating that the increased vertical lifting is dominating the increasing photolysis from the ozone decrease. After 2020, the CFC-11 relative concentration begins to decrease

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Interactive Discussion

Discussion Paper

as increased UV photolysis begins to dominate. Shortly after 2055, when the tropical ozone drops rapidly, the photolysis effect strongly dominates, and the relative concentration of CFC-11 in the tropics decreases rapidly increases. At 30 hPa the relative fraction is about 0.3 in 2055 and decreases to about 0.18 by 2060. At 50 hPa the relative fraction is about 0.7 in 2055 and decreases to about 0.65 by 2060. At 70 hPa, the change in the relative fraction is about 0.9 and shows only a tiny decrease between 2055 and 2060.

The effects of $O(1D)$ and OH are not illustrated by CFC-11, but may have important consequences for other molecules. Both $O(1D)$ and OH increase with time in the tropical lower stratosphere of the simulation until about 2055. By 2055 the OH concentration is nearly double its concentration at the beginning of the simulation. $O(1D)$ begins increasing in about 2010 until its concentration is about 50% greater in 2055 than at the beginning of the simulation. Both then drop rapidly (OH by about 50% and $O(1D)$ by more than a factor of 5 as the ozone concentration falls to near zero levels.

While we provide this material as background to our answer, we have opted to not include this in the body of the paper because we felt that it is too specialized.

> *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?*

The model does an excellent job of capturing the tropical temperatures. See Fig. 3 in Pawson et al. (2008), which shows the tropical temperature at 100 hPa. Pawson et al.

ACPD

8, S11659–S11667, 2009

(2008) state, “In the Tropics, differences between run P1 and observations are within tenths of a degree.” The heterogeneous chemistry appears at approximately the same time at 70 and 50 hPa.

> *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 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.*

The increase in the tropical upwelling is driven by the increased eddy momentum flux convergence in the subtropics. This momentum flux is in turn a result of the alteration of the index-of-refraction that is a result of the increase of the zonal mean flow at the top of the subtropical jet (70 hPa, 30°N). The acceleration is in turn a result of the differential heating of the lowest-most stratosphere between the tropics and extratropics—lack of ozone in the midlatitudes cools the 300–100 hPa layer (less shortwave absorption), while the layer in the tropics remains about the same. Hence, the heating and then the wave mean flow interaction cause the accelerated lifting.

The eddy effects in the 2-D model are simulated using the parameterization of Garcia (1991). Hence, the 2-D model captures the essential details of the zonal mean shortwave heating, temperature cooling, and zonal mean wind changes observed in the 3-D model. The eddy parameterization then simulates the increased momentum flux into the subtropics, and this momentum deposition accelerates the lifting.

In summary, the 2-D model includes all of the basic physics in the 3-D model, and then

ACPD

8, S11659–S11667, 2009

Interactive Comment

adequately parameterizes eddy effects of the wave-mean flow interaction. Hence, the 2-D model closely follows that basic physics that we simulate with the GEOS-4 CCM.

We have added a bit more discussion on the 2-D model to clarify the agreement.

> *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).*

The two hemispheres do indeed behave differently. The eddy forcing in the SH is overwhelmed by the lack of shortwave heating. In the NH, the eddy forcing dynamically heats the stratosphere, resulting in very similar temperature effects in mid-winter and a reversal of the zonal mean wind to easterlies in the spring. We have added some text.

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

Fixed.

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

Agreed. Changed.

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

Done.

ACPD

8, S11659–S11667, 2009

Interactive Comment

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

Agreed. Changed.

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.

Thanks!

Benedick, R. E.: Ozone Diplomacy, Harvard University Press, Cambridge, Mass., 1991.

Farman, J. C., Gardiner, B. G., and Shanklin, J. D.: Large losses of total ozone in Antarctica reveal seasonal ClO_x/NO_x interaction. Nature, 315, 207–210, doi:10.1038/315207a0, 1985.

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ACPD

8, S11659–S11667, 2009

Interactive Comment