

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. Harris for his thoughtful comments. In this response, his comment is indented, and our response immediately follows.

1. Ozone loss.

The authors use the term ozone loss in a somewhat vague way. Loss has come to mean chemical loss, but it is not clear that that is how they mean it here. If they cannot diagnose chemical loss, then they probably need to be more careful in their language, with at least a definition of terms earlyish in the discussion. This is worth clarifying, because a perfectly valid argument could be made that it does not matter whether the loss is chemical or dy-

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namical in origin because it results from the presence of such high levels of CFCs. If that is the authors' position they should state it.

Agreed. We have inserted the following text into the last paragraph of the introduction: "The large CFC perturbations used here lead to large chemical ozone losses and large dynamical changes that lead to large ozone changes. In this study, we have not attempted to separate these chemical and dynamical effects, but refer to them together as ozone losses."

2. Separation of chemistry and dynamics.

I raise the first point partly because section 6 on dynamics and transport is presented in a somewhat linear way (less ozone -> more/less heating -> different gradients, etc) without much mention of how these processes are feeding back on ozone as well as each other. If surface UV radiation is the only end product, this does not matter much, but this seemed one place where the generally clear presentation probably glossed over some interesting science and became somewhat 'lite'.

We made a deliberate choice to only bring out the higher level points of the paper, and leave some of the more interesting feedback processes to a more thorough and pointed dynamics paper on the world avoided.

3. Model dimension.

The statements about the 2D and 3D models were interesting (as Drew Shindell has also noted). If the conclusions about similarity of 2D/3D models are robust, then it implies that much greater use could be made of 2D models in UNEP/WMO assessments at least for the quantities (ozone, chemistry and UV) which are the main subjects of this paper. I imagine

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that 3D stratospheric modellers would have much strong views about this prospect, but I think this aspect could usefully be investigated more here (or I guess another piece of work). Such a conclusion is feasible as much of the discussion in the text is about altitudinal and latitudinal gradients rather than longitudinal ones.

This good agreement between the 2-D and 3-D models is not surprising in light of previous studies which showed that the zonally-averaged stratospheric ozone and tracer fields simulated by 3-D models were well represented by self-consistent 2-D model simulations on time scales of 30 days or longer (e.g., Plumb and Mahlman, 1987; Yudin et al., 2000). Also, our previous work has shown that a 2-D model framework can successfully reproduce many of the transport-sensitive features seen in a variety of stratospheric ozone and tracer observations (Fleming et al., 1999). The good 2-D/3-D model agreement also illustrates that the propagation and breakdown of planetary waves in the stratosphere, and the related interactions with the zonal mean flow, are well represented by the linearized planetary wave parameterization used in our 2-D model (Garcia, 1991), even in this highly perturbed *WORLD AVOIDED* scenario (see our response to Drew Shindell's comment on the tropical upwelling). The nonlinearities of these wave-mean flow processes, as simulated in the 3-D model, are relatively unimportant on the long time scales (> 1 month) pertaining to this study.

We have modified the text discussing the 2-D model.

We note also that since the submission of this paper to ACPD, we have further improved the 2-D model to be more consistent with the GEOSCCM. We now include in the 2-D model consistent long-term changes to the model surface temperature, and latent heating and water vapor in the troposphere due to the changes in atmospheric CO₂ loading and SSTs. These are based on the sensitivities of these quantities to CO₂ changes as derived from the GEOSCCM reference simulation for 1950–2100. When including these additional parameterizations, the 2-D model global total ozone time se-

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ries in Fig. 2 is very close to that of the GEOSCCM after 2000. We have updated our Fig. 2 to include this new 2-D simulation.

4. Figures.

The plots in general are clear and well thought through. However, I doubt that they will be clear (particularly the inserts) when printed in the final size. The authors do therefore need to reconsider how they are presented in the published paper. However, for most readers, it would be good if the current versions could also be made available as they are the best for use in talks or lectures.

We have modified the figures to be slightly more readable by: 1) increasing the font sizes, 2) splitting out the inset in the tropical ozone figure, and 3) thickening contours and increasing font sizes on contour labels.

It is not clear to me whether figures 8 and 9 are strictly necessary. The main points are that active chlorine becomes negligible in the Arctic (at least in March) by 2040; and that active chlorine suddenly becomes important after 2050 in the tropics. This is said clearly in the text. The authors should at least consider whether full speciation is needed for the chemical species.

The figures are all meant to visually reinforce points made in the text. Figure 8 (now Fig. 9) shows the increasing levels of Cl_x up to about 2020, with a consequent fall in Cl_x after 2030 as ozone goes to a zero value. Such behavior is already evident in the SH. Nearly the entire paragraph on P. 20579, l. 2-22 was devoted to discussing the details of Fig. 8. Figure 9 (now Fig. 10) shows the changing partitioning of Cl_y as the temperatures cool (from Fig. 7). The paragraph (P. 20579, l. 23 to P. 20580, l. 10) has an integrated discussion of Fig. 7 and Fig. 9. We felt that both figures provide useful information that supports the chemistry discussion.

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I also struggle a bit with Figures 10-12 which seem far more complex than the associated discussion (see my comments in 1 and 2).

We have increased the discussion with respect to a number of features in the figures.

Technical Corrections.

20566, 5: ...and modelling studies. This research led...

20569, 27: must? The actual success of the MP is best measured against....

20575, 1: precipitously over 90 years? Dramatically?

20583, 19/20: End this sentence end after 'globe'? Personal exposure is not greatest in cities and I would hope that most people know where Washington is.

20588, 1: Newman, P.A.

All fixed.

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