**Atmospheric
Chemistry
and Physics
Discussions**

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 the referee for the thoughtful comments. In this response, the referee's comment is indented, and our response immediately follows.

> *(a) Can you formulate quantitatively what people in the 1980s thought the ozone loss might be, and compare to the GEOSCCM results?*

Yes. We have added some text about expected losses from the 1979 NASA reference publication.

> *(b) Can you formulate "theoretical expectations" of how the model should*

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behave in a high-chlorine world, and validate these using the model results?

This model simulation is a theoretical expectation that was (fortunately) never realized. The model is a tool for incorporating the complex interactions that we understand into a "theoretical expectation" of how the atmosphere might respond. If the reviewer means by "theoretical expectations" something much simpler (e.g., what one could do on the back of an envelope?), then we would argue that the model validates some of these expectations and gives us a little bit richer view of others. For instance, we expected that ozone would decrease as chlorine increased, and the model simulation does that. We did not expect the tropical lower stratosphere temperature decrease leading to the rapid tropical ozone collapse. Thus, the model enriched our understanding of the complex interactions in this scenario. In retrospect it is rather easy to understand what happened in the tropical lower stratosphere and, in a post-hoc sense, fold these results into our new "theoretical explanations". We realize that this does not mean that the atmosphere would necessarily behave this way, but it does make sense for the information that we fed to the simulation.

(c) Indeed plotting EESC vs total ozone is a good way to display the re-sult which can partially be validated against observations, albeit for a much more limited range of values.

The Cl_y predictions of the model and a Cl_y estimate from observations are shown in Fig. 1 of Eyring et al. (2007) and an HCl model and observations figure is shown in Fig. 11 of Eyring et al. (2006). These comparisons show the GEOS-4 does a very good job of representing chlorine in the atmosphere. EESC is the sum of Cl_y and Br_y, with Br_y scaled upward by a factor of 60 to account for the greater efficiency for catalytic ozone loss. Hence, strictly speaking, we can't compare EESC to observations since we don't observe Cl_y and Br_y. Comparisons of ozone observations to the model

are shown in both Eyring et al. (2007) and Pawson et al. (2008). Both papers show excellent agreement with Cl_y and ozone observations.

We have added discussion in the Model Description section to highlight this good comparison.

(d) Did you actually discover "subtle non-physical model problems"? If so, please tell us. I think on the whole these questions are slightly out of balance with the rest of the paper, so I would restrict them here to what you actually address in the paper.

Subtle problems were discovered in the 2-D model involving the total nitrogen budget and the implementation of the troposphere (see the discussion in response to Dr. Harris' question about the 2-D model for a discussion of the tropospheric adjustments). The 3-D model had an easily resolved problem with varying the time-step of the chemistry to very short values (<5 minutes) as chlorine increased to large values (>35 ppb). The ability of the model to handle these extreme perturbations gives us good confidence in the results for more modest perturbations such as the low chlorine simulation. However, discussion of these problems is a bit mundane for the paper, and we have dropped this rationale from the Introduction.

p 20574 l 5-7: This way of defining EESC may be inappropriate as you move into a high-chlorine world. In the present-day atmosphere, possibly adding a small amount x of bromine has got a similar effect on ozone depletion to adding 60x of chlorine. However, the BrO + ClO cycle links the effects of bromine to the amount of chlorine in the system, so in a high-chlorine atmosphere bromine becomes more efficient at depleting ozone. I accept that this is not the main focus of the paper, but a few words about the limitations and simplifications involved in defining EESC would be in order.

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The referee is correct that the scaling factor will be total chlorine (and total bromine) dependent (see Fig. 4 of Danilin et al., 1996). Furthermore, the relative importance of Br_y (represented by alpha) is variable between latitudes and altitudes. Both factors make a difference in the EESC that we would calculate when both bromine and chlorine get very large near the end of the model simulation. We use the calculated EESC mainly as a guide to give the reader an understanding of how much chlorine and bromine that we have added to the atmosphere. The model simulations include ozone loss determined from the calculated concentrations of ClO, BrO, and other radicals without reference to EESC. We have included some brief comments on this point in the revised manuscript.

Also, what do you do to bromine in the simulation? Does it grow with chlorine or remain at present-day levels?

Halons and methyl bromide also grow in our *WORLD AVOIDED* simulation at the 3% per year rate. We have added text to clarify this point in our section on the simulations.

p 20575 l 16: Some readers may be perplexed to find that ozone depletion is linear in EESC when the dominant reaction involved in ozone depletion is quadratic in chlorine. Could you expand why there is this linearity?

The $ClO + ClO$ reaction (Molina and Molina, 1987) has been cited as the quadratic rate-limiting step, and indeed this is the important reaction for ozone loss in PSC dominated regions. It is assumed that this catalytic cycle should also increase as the square of Cl_y. However, this assumes that ClO increases linearly as Cl_y increases linearly (i.e., the partitioning remains fixed). We have examined that normalized partitioning of ClO and $ClOOCl$ as a function of year for the Antarctic spring period. The ClO/Cl_y ratio decreases from the 1970s to the 2000 period. Hence, ozone loss is not proportional

to Cl_y^2. Further, the rate-limiting step for global losses is generally the Stolarski and Cicerone (1974) reaction, which is linear in Cl_y levels.

p 20581 l 19 etc: Presumably the age tracer is kept at 0 at the Earth's surface? And by "time difference" you mean "age difference" between any given point in the atmosphere and the tropical tropopause.

The age tracer is fixed to the time at the surface (in years), and the stratospheric age is calculated with respect to the difference between this age tracer and the value at 100 hPa between $10°$S and $10°$N. Text modified to clarify this point.

The figures are easy to read and very helpful in understanding the paper. In figure 2, in the EESC vs ozone insert, observational data could be included for comparison. The same goes for the insert into figure 7 (T versus ozone).

See response to Comment (c) above.

Stolarski, R. S. and Cicerone, R. J.: Stratospheric chlorine: A possible sink for ozone, Canadian J. Chem., 1616–1624, 1974.

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