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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Options to accelerate ozone recovery: ozone and climate benefits" *by* J. S. Daniel et al.

Anonymous Referee #2

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This manuscript examines options for modifying the recovery of stratospheric ozone, including the option of regulating N2O. This is a very interesting and topical paper, and the results will be of interest to many people. I think it warrants publication in ACP, however I think revisions are required before it can be accepted. As described below there are several issues that need further clarification and discussion. Whether these changes are viewed as minor or major will depend on the answer to comment 1.

MAJOR COMMENTS

1. Are the models forced with emissions rather than mixing ratios? I couldn't find this stated anywhere, and this is a very important issue.

To do the analysis correctly emissions should be used. However, typical 2D model runs

(e.g. for Ozone assessments) use mixing ratio boundary conditions, so I am not sure this is the case. If emissions are indeed used then there needs to be some discussion of differences in surface mixing ratios in the simulations. See also impact points 3 and 5 below.

If mixing ratio boundary conditions are used then this presents a major inconsistency in the analysis. Where have these boundary conditions come from? If another model (e.g. 12-box model), then the mixing ratios are likely not consistent with those that would result if emissions were used in the 2D models shown. The mixing ratios from given emissions will vary depending on the circulation, which changes with time and differs between the models. Or stated another way, imposing the same mixing ratios implies different emissions between models. If mixing ratios boundary conditions are used then there needs to be a major change in the discussion of the calculations / results.

2. Why is N2O considered an ODS but not CH4? Changes in CH4 will result in changes in stratospheric HOx, and hence ozone loss via the HOx catalytic loss cycle. Actually, its not even clear to me why CO2 is not also considered. If you are to expand beyond anthropogenic halogens then I think need to consider all anthropogenic gases that will alter the ozone distribution, even if some of them turn out to have a positive impact. Only including gases with a negative ozone impact is like IPCC considering only anthropogenic changes with a positive radiative forcing. (CO2 will clearly have a positive impact through its cooling effect, but it is not clear to me why CH4 will have a positive impact. This comment needs some support.)

Even if the authors don't agree with me regarding the above issue, the issue needs to discussed up front (e.g. introduction, and in discussion of the runs) and not buried in Section 3.

3. I think there needs to be time series plots of the emissions (total chlorine, bromine and N2O) together with plots of surface mixing ratios, for the different cases. As it is I

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found it hard to keep track of differences between different cases (and to know exactly how different the mixing ratios would be). A figure could get the differences between cases over a lot clearer than the table.

Also, I assume the mixing ratios will differ between the models (if forced with emissions). If they are not different then this alone is an interesting result.

4. I am confused about what age is used in the EESC calculation. Initially it is stated that 3 yrs. is used, but then later it appears the model ages are used. I think the latter is the most appropriate, and the comments saying 3 yrs and 1.5 years are used for age and width should be replaced with statements saying values appropriate for the models is used. Also, the comment about choosing a width to match ozone best should be in the main text and not buried in caption.

What EESC is shown in figure 1? As only one curve does this mean 3yrs was used for the mean age? Two sets of EESC should be shown just as there are 2 sets in the upper panels.

I think it would also be useful to show simulated Cly + alpha*Bry + factor*NOy term in lower stratosphere, rather than just EESC. Does this differ from EESC?

5. There are statements about how well the models perform in simulating ozone and regarding trends but no plots. This paper only has a few figures, and I think a figures showing distribution of o3, cly and age (and maybe trends) would be very useful. This would allow the reader to determine if the agreement with data or other models is "good".

The comment takes on more importance if models are forced with emissions rather than mixing ratios.

MINOR COMMENTS

pg. 10841, Line 14: "These CCl4 sources ... reported production" This statement, and perhaps some earlier ones, needs a reference. The second paragraph of the

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introduction makes a lot of statements with no references for the reader who is less familiar with the emissions and banks of different CFCs.

pg. 10842, Line 18: Why are old estimates of HCFC release rates, based on models that are no longer state of the art, continually used rather than estimates based on observations? Yes there is a lot of uncertainty, but I see no justification for ignoring observations and using old model results.

The discussion in first part of Section 3 is rather confusing as the background value used in the different discussion varies. There is discussion of recovery to background values, but what background is not defined (it appears to be values before 1950). Then comparisons are made to other estimates using ∼1970 and 1980 values for background. If these comparisons to other studies are to be included then I think you need to calculate the changes in your model using the same reference period as these previous studies. If just a difference in definition then differences would go away.

Figure 1: I think the rescaling of NOCAR needs to be mentioned in the text.

Figure 2: Same as above for age spectra fit.

Figure 3: Which model is shown? What age is used for EESC?

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 10839, 2010.