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Atmospheric Chemistry and Physics Discussions



# *Interactive comment on* "A model study of the impact of source gas changes on the stratosphere for 1850–2100" *by* E. L. Fleming et al.

**Anonymous Referee #1**

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This is a very well-written paper. It is a textbook example of how sensitivity simulations could be used to obtain diagnostic information to quantify how different processes affect the model results. With the 2-D model results evaluated using observations and results from the GEOSCCM, it provides some confidence that those processes reflect reality.

I would like to raise the following issues for discussion. I think some of them could be addressed in future papers if the authors feel that they are out of scope of the current paper. (1) In section 3.4, the CH4 sensitivity experiment is described as 0.5 ppmv CH4 perturbation for the year 2000 conditions. Is this a steady state simulation using annual repeating transport parameters, or is it a 1-year time slice. I assume it is the former. It may make a bit of difference in the ozone response in the UT/LS, and the free troposphere. (2) The simulations in section 3.5 are clearly described, except for

the CO2. For CFC-11, N2O and CH4, one can indeed do a steady state run. In this case, either mixing ratio boundary condition or flux boundary condition should give you the same answer. However, this is difficult for CO2. Was it simply assumed that there is a uniform mixing ratio change for CO2 and use the burden difference to derive an emission rate assuming a lifetime? This will make the connection to ODP somewhat difficult to make. (3) It would be very useful to give the steady lifetimes for CFC-11, N2O and CH4 either in Table 1, or in Figure 13 so that they can be compared with the instantaneous lifetimes in figure 13. (4) Would forcing at the tropopause be useful numbers to have in section 4? (5) The results on instantaneous lifetimes in section 6 are very interesting. Again, it would be useful to have some steady state lifetimes from the section 3.5 simulations for comparison. It is a bit surprising to see the "green curves" stay constant after 2000. The surface mixing ratio from IPCC are estimated using the WMO lifetime and emission from banks. The assumed lifetime is different from the model lifetime. Is the result telling us that as long as the lifetime is sufficient long, the relation between the stratospheric distribution and the surface mixing ratio will be similar to the steady state relation?

Minor (1) Please add a sentence in section 5 to refer to Appendix B2 on how the age of air is calculated.

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