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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Atmospheric lifetimes and ozone depletion potentials of trans-1-chloro-3,3,3-trifluoropropylene and trans-1,2-dichloroethylene in a three-dimensional model" *by* K. O. Patten and D. J. Wuebbles

Anonymous Referee #1

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This is a straightforward paper reporting on the calculated ODP for two very short-lived species (VSLS). While I have no argument that the ODP values for the 2 compounds are small, there are some interesting questions to which I would like to hear the authors' responses.

(1) Everyone recognizes that the ODP value is also dependent on time of the year when the emission occurs. It is strange that the authors continue to use uniform emission throughout the year, rather than doing 4 seasons. It would be more work, but will provide useful needed information. (2) If a VSLS molecule enters the stratosphere, it

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should release its chlorine atoms near the tropopause. Thus, to a very good approximation, there are no local sources and sinks for Cly in the stratosphere and changes in Cly mixing ratio due to the VSLS should be uniform. Yet the results in Figures 2a and 2c show large latitudinal and vertical gradients. One explanation may be that there is such a large gradient in Cly across the extra tropical tropopause that there is diffusive transport in the model to maintain the gradients. There are publications that argue that the entry point to the stratosphere is predominately through the tropical tropopause. Is this diffusive transport caused by the high concentration in the extra-tropical troposphere realistic? (3) In order to get the 1% depletion in the stratosphere, the model must be forced with a high emission, resulting in large increase in Cly in the troposphere. Does this corrupt the ODP results? Should depletion in the troposphere be included in the ODP calculation? (4) Is the ozone response to Cly increase in the troposphere linear or does it saturate? If it saturates, are we underestimating the tropospheric ozone depletion per unit mass emitted by using such a large emission? (5) If the ozone depletion is mostly in the troposphere near the emission region, is the concept of using a single number to characterize ozone depletion still useful?

Other technical comments (1) The statement that much of the ozone loss occurs in the troposphere (50% for tCFP and 96% for tDCE) should be included in the abstract. (2) P. 16643 and p. 16647: On p. 16643, the authors stated that the large flux could lower (my word, the authors used the word “alter”) the OH field in a CTM, resulting in overestimation of atmospheric lifetime. The authors then continue to state that this is due to reduction in OH production associated with ozone depletion. On p. 16647, the authors suggest that the high fluxes may saturate the oxidizing capacity in the troposphere, suggesting that OH is suppressed because of the enhanced loss associated with the reaction with tDCE. Which is it? (3) P. 16645. The authors claim that comparison of figure 1a and figure 2a suggests that half of Cly increase is due to Cly transported from the troposphere. I do not think this is justified because in figure 1a, the mixing ratio of the source gas along the tropopause ranges from 25 to 100 ppt, and change in Cly is not uniform in the stratosphere. (4) What is the reason for the calculated decrease in

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ClOx in figure 2?

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 16637, 2010.