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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Transport and modeling of stratospheric inorganic chlorine" *by* D. W. Waugh et al.

Anonymous Referee #1

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GENERAL COMMENTS

This paper analyzes the relation between stratospheric inorganic chlorine (Cly) and the mean "age of air" and other transport factors. Cly is the primary driver for stratospheric ozone depletion by chlorine containing species and therefore the future evolution of the ozone layer will critically depend on the evolution of Cly. A realistic simulation of Cly by chemistry climate models (CCMs) is therefore crucial for modelling past and future ozone levels. The work presented here is a highly welcome contribution shedding light onto the question to what extent different representations of transport contribute to the large model-to-model differences in Cly levels (and accordingly ozone evolution) reported in previous CCM assessments (Eyring et al., 2006 and 2007).

The results are based on three different model simulations. Two of them used the same CTM (identical advection scheme and chemistry solver) and the same halogen boundary conditions (predefined concentrations in lower troposphere) but with different horizontal grid resolution ("high" and "low"). The third simulation was identical to the "low" simulation above but used a different meteorological input. Based on this simple but adequate setup the authors demonstrate in a concise way the effect of different model resolutions and of different meteorological fields driving the CTM on the distributions of age of air and Cly. The study convincingly shows that not only the mean age of air but also the fractional release of reactive chlorine from the chlorine source gases is important to explain the simulated Cly levels. The latter is related to the transport pathways and specifically to the time the air was exposed to UV light in the middle and upper stratosphere. Even though this is not a new conclusion per se (cf. paper by Schoeberl et al. (JGR, 2000) which is correctly cited) it is to my knowledge the first study highlighting this fact based on an Eulerian model approach and is thus directly relevant for other CCM studies. The paper concludes with the important statement that "comparisons of simulated mean age and CFCs (or directly Cly if possible) with observations provide complementary tests to assess the transport time scales and transport pathways in the models." I fully agree that CCM models should demonstrate that they are capable of simulating these two aspects in a realistic way.

Current CCM projections of the future evolution of stratospheric ozone (see WMO, 2007) are based on prescribed concentrations of the halogen source gases in the troposphere. As stated on page 6.36 of the WMO report this neglects relevant feedbacks which may alter the lifetime of the compounds. The realistic simulation of stratospheric Cly is mandatory for a next generation of simulations in which only the surface emissions would be prescribed and the atmospheric lifetimes would become an intrinsic element of each model. This will likely reveal even larger differences between the models and the realistic simulation of Cly will become even more important, and this paper will serve as a reference.

The manuscript has only a few (mildly) weak points:

1. The paper title is fairly general. A title like “Sensitivity of stratospheric inorganic chlorine to differences in mean age and transport pathways simulated by chemistry climate models” would probably better reflect the scope of the study.

2. Differences in model simulated Cly are only one (though important) factor contributing to the differences reported by Eyring et al. (2006; 2007) or by WMO (2007). Other factors are differences in the treatment of external forcings like solar cycle and QBO or differences in the radiation schemes and radiative feedbacks included. The response of stratospheric temperatures and the stratospheric circulation due to climate change differs between models which clearly affects the evolution of stratospheric ozone. This should be mentioned in the introduction which emphasizes too strongly the Cly aspect.

3. The model simulations should be described in some more detail. Since only a single year was used repeatedly in the simulations it would be good to know which year it was in the case of the DAS simulation. For the GCM simulations, on the other hand, it would be good to know in what phase of the QBO and solar cycle the selected year was (if these forcings are included at all). Some differences between the DAS and GCM simulations may arise from the specific selection of years. How is radiation treated in the simulations? Was the same radiation scheme used (I suppose the GCM has its own radiation code)?

4. The year when Cly (or O3) returns to 1980 levels is often used as a benchmark to highlight differences between models. It is a pity that the simulations seem to be too short to demonstrate the differences with respect to this important measure (which otherwise could have been indicated in Figure 4). If possible, this should be added.

5. The titles of Sections 3.1 and 3.2 should be changed. Naming them “CTM simulations” and “Theory” does not adequately reflect their content. My suggestion would be: “3.1 Simulations of Cly and mean age” and “3.2 Fractional release rates as a diagnostic for transport pathways”.

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SPECIFIC COMMENTS AND TECHNICAL CORRECTIONS

- Last sentence of abstract: “provides” -> “provide”

- Page 8599/line 9: I disagree that the radiation schemes are very similar. There are important differences in the way the different species are coupled to the radiation scheme (see Table 1, column “radiative feedback” in Eyring et al., 2006) and also the number of spectral bands considered is probably quite different between models. These differences likely contribute to the differences in the simulated ozone response to changes in CH4, N2O, H2O and CFC concentrations which are not in all models interactively coupled to the radiation scheme.

- Page 8600/line 9: “some of the simulations” is too general given the fact that there are only 3 simulations. Suggestion: “We consider three different simulations with one pair differing in the meteorological fields driving the CTM and one pair differing in horizontal resolution and model top.”

- 8601/22: What are the reasons for the large differences between DAS-LOW and GCM-LOW? The paper by van Noije et al. (JGR, 2006) highlights some of the problems of assimilated data sets with respect to simulating the stratospheric circulation. The paper by Schoeberl et al. (JGR 2003) on stratospheric age spectra also addresses the problems of the DAS assimilated data set.

- 8603/16: I don’t understand what is meant by “the method of Schauffler et al. (2003)”. As far as I can see the method of Schauffler is already described by equations (2) and (3). Or do you refer to the specific calculation of a polynomial fit through the fractional release rates F_i and mean ages used by Schauffler to describe the functional relationship between the two?

- 8603/21: Change sentence to "the CTMs, and the above estimates of F_i based on the functional relationship with gamma”.

- 8606/7: “becomes” -> “become”

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Interactive comment on Atmos. Chem. Phys. Discuss., 7, 8597, 2007.

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