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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 #3

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General Comments:

This paper makes use of three simulations from the same off-line chemical transport model to study the role of transport on the simulated Cly in these runs. The paper is well structured and provides important conclusions on the importance of pathways in addition to mean age of air. These kinds of process studies are highly needed in addition to the analysis of multi-model long-term simulations that are used and analyzed in support of international ozone and climate assessments. The only critical comments I have are that the paper could be a bit clearer with respect to what these findings mean for EESC and Cly* as calculated by Newman et al. (2006, 2007), and that the text could be improved by given the reader a bit more information and by using a more precise wording in some places, see comments below. After these comments are considered

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I can recommend publication in ACP. I am convinced that this study fits well within the focus of ACP and that this paper will be a frequently cited work that helps interpreting differences in simulated Cly among models in the future.

Specific Comments:

Abstract (page 8598):

- line 5/6: 'with these differences explaining differences in the simulated evolution of ozone over the next century.' I doubt it is only Cly that explains all the differences in ozone among the models, but I agree it is one of the main reasons. How about changing this sentence to 'with these differences explaining many of the differences in the simulated evolution of ozone over the next century, in particular in Antarctica, but also in mid-latitudes.'?

- Could you consider adding a statement what your findings mean for the calculation of EESC and Cly* following Newman et al (2006, 2007)?

Introduction:

- page 8598, lines 21-23: While I agree with the second sentence of the introduction that modeling Cly and Bry is crucial for modeling past and future stratospheric ozone levels, I disagree with the first. Eyring et al (2007) concluded that 'In general, the simulated ozone evolution is mainly determined by decreases in halogen concentrations and continued cooling of the global stratosphere due to increases in greenhouse gases (GHGs).', so it is not just Cly and Bry. Please rewrite.

- page 8599, lines 2-4: 'Furthermore, these differences appear to explain the differences in simulated ozone between the models, e.g. models with a later return of Cly to pre-1980 values also have later return of ozone.'. Please rewrite, e.g. change to 'explain many of the differences' and 'also in general have later return of ozone'.

- page 8599, lines 7-9: Radiation and chemistry schemes as well as the coupling of GHGs other than O3 and H2O differ quite a bit among the models used in Eyring et

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al (2007). Even the photolysis of CFCs is not the same in all the CCMs, as e.g. in one model Cly production rates are parameterized according to the age of air and the tropospheric source molecules as a function of time. While all your arguments made in the next sentence on the importance on understanding differences in Cly still hold, please be more precise in the description here.

2. Model description:

- I think a table that summarizes the main features of the three simulations would help. Some more details could be added, see e.g. below.

- page 8600, lines 19,20: 'In each case a single year of meteorological fields is used '. Which year?

- page 8600, lines 23-26: This is quite an important information that shouldn't be hided in a sentence in brackets. When looking at the Cly and Clytot evolution in Figure 3 it is surprising that the Cly and Clytot values in 1995 are higher than in 2000 in the two LOW simulations. Or is this because of other reasons?

- page 8601, lines 1: A bit more detail on how the age spectrum was calculated would help. Did you use a conserved tracer with linearly increasing concentration? Which tracer, SF6 or CO2 or any other method? Additional twenty years: which years?

3. Results:

- Do your conclusions also hold if you use a 10 year mean rather than a single year (or a mean of 1995, 2000, and 2005)? It would be interesting to know, as e.g. model evaluations are often based on more than one year.

- page 8601, line 13: surface concentrations of organic chlorine species

- page 8602, lines 14-20: mention overestimation of Cly in the two LOW simulations in 1995 because of spin-up.

- page 8602, line 24: following 'determining Cly.' Please add a bit more explanation,

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e.g. altitude dependence of CFC photolysis, CFC decomposition depending on the time in the stratosphere and thus importance of age of air for correct estimates of Cly and Bry.

4. Theory:

This section needs to be improved for clarity. It is not always clear what is meant - some suggestions are below.

- page 8603, lines 4,5: 'and Fi the fraction of source gas i that has been dissociated in the stratosphere relative to the amount that entered at the tropopause.'

- page 8603, line 11: 'at a given stratospheric location', i.e. dependent on latitude and altitude? Please clarify.

- page 8603, line 13: where the mean-age-of-air is a function of what? Latitude and altitude? It is unclear what is used in the calculations.

- page 8603, lines 16-18: Schauffler et al. (2003) used ER-2 observations to calculate the fractional release of CFC-11 as a function of mean age-of-air. Is this meant here or are you using the actual model fields to determine the fractional release rates and apply equation 3? If the latter, the wording is confusing.

- page 8603, line 19: for clarity, please change e.g. to 'We first compare Cly* as calculated by applying equation 1 to the simulated fields with the actually simulated Cly in the three model simulations.'

- EESC is a frequently used parameter to quantify the effects of chlorine and bromine on ozone depletion in the stratosphere and any uncertainties in the calculations can lead to misattribution. A paragraph in this section should be added that summarizes what your findings mean for EESC and Cly* as calculated by Newman et al (2007) using observational based fractional release values, as otherwise the reader has to puzzle this out. E.g., what can be concluded if Cly* and Cly are very similar? Cly* only depends on mean age of air in your calculations, but not on the pathways. Also, differ-

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ences in transport pathways can result in differences in the fraction of air that has been in the upper stratosphere, where organic chlorine is converted into Cly. In a changing climate, transport pathways might change, which could be another uncertainty in EESC and Cly* projections.

- page 8603, line 21: 'and the above estimates of Fi calculated from model fields'

- page 8603, line 25 - page 8604, line 4: Another important sentence in brackets. Could you consider adding the Cly* estimate that is calculated by Newman et al (2006, 2007) for the mean age of air simulated in each of the simulations, but with fractional release rates based on observations? Or are these the dashed lines in Figure 4? Could you consider adding Cly from observations to Figure 3 and 4?

- Figure 1: add the year to figure caption

- Figure 4: What are the dashed lines? Please add to figure caption. Can you add observations?

Minor Comments:

- Replace 'age' and 'mean age' with 'mean age of air' in entire manuscript

- Probably 'old age' should be changed to 'high mean age of air' and 'young age' to 'low mean age of air'?

- For readability of the manuscript, please avoid long sentences in brackets (e.g. page 8600, lines 23-26; page 8601, lines 19-20)

- Again to improve readability, please write out 'fractional release rates' and 'mean age of air' rather than using the symbols in the text.

- page 8598, line 26: WMO, 2007: which chapter?

- page 8601, line 11: 'in mid- and high latitudes'

- page 8606, line 7: 'becomes' should be 'become'

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