

***Interactive comment on* “Semi-empirical models
for chlorine activation and ozone depletion in the
Antarctic stratosphere: proof of concept” by
P. E. Huck et al.**

Anonymous Referee #1

Received and published: 19 December 2012

General comments

The manuscript introduces two semi-empirical models to describe the evolution of the vortex averaged ClOx and ozone mass deficit (OMD). The authors show that their simplified but physically based models in combination with statistical fitting are able to capture the observed variability of ClOx and OMD during the last 30 years. The subject of the manuscript is relevant to the ACP scope and potentially interesting for the community, because it could provide an inexpensive way to forecast the behavior of the considered quantities in the future. I think the publication of the manuscript can be recommended. However, there are several issues in the manuscript (see below) and

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maybe some moderate revisions will be necessary before the publication.

Specific comments

1. The authors use FAP which is based on the temperature threshold for NAT PSC formation, but the liquid aerosol seems to be more important (e.g., Wegner et al., 2012, ACP) and the authors should briefly discuss this issue. The ICE PSC also should play an important role in the formation of ozone hole; however its contribution is missing in the proposed parameterization. Does the good agreement with observations means that these processes are not important for the ClOx and OMD evolution?

2. The authors state: “Heterogeneous reactions on PSC surfaces lead to the conversion of reservoir forms of chlorine into active forms (Solomon, 1999, and references therein). Once reactive chlorine is exposed to sunlight, chlorine-catalysed ozone destruction begins...”. However, the heterogeneous reactions do not lead to immediate chlorine activation. During the first stage Cl₂ reservoir is formed and the presence of sunlight is necessary to convert it to ClOx. Potentially, the Cl₂ formation is possible for FAS=0 and ClOx formation could act even if FAP=0. Does it have any implication for the proposed parameterization? The equation (1) will not produce any ClOx if these two processes are sequential, and even for the time periods when FAP=0. I think it would be helpful to show the applied evolution of FAP and FAS from eq.1 at least for selected time periods.

3. The amount of Cly in the proposed parameterization does not depend on the actual meteorological situation, however it does in reality due to interannual variability of vertical descend and cross vortex mixing. In turn, these quantities depend on the ozone sate (especially for the future climate). What implications for the forecast could have such a simplification?

4. The authors suggest that the removal of NO_x is parameterized using FAP, but it should be noted that NAT particles are too small and substantial sedimentation takes place only if they are inside ICE particles, therefore the irreversible denitrification could

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be substantially overestimated.

5. The authors suggested three processes responsible for the ClOx deactivation, but only one is included in the parameterization. What about other processes? Their intensity depends on the presence of the sunlight which is not included in the equation (1).

6. It is stated that: "The model tracks the observations for activation and deactivation of ClO well". It would be useful to provide some quantitative measure how well is the agreement. Is it superior compare to the CCMs performance? The opposite trends in the observed and parameterized maximum ClO values are visible from the Fig.1. Is there any explanation of this behavior?

Minor comments and technical corrections: 1. Page 28456, line 2: I would use "over" instead of "from".

2. Page 28456, line 21: Which CCM were used? Probably the model should be briefly introduced.

3. Page 28457, line 1: I do not understand why "...only temperature fields are required..". Cly is also necessary.

4. Page 28458, lines 21-26: The description of Fact is not satisfactory.

5. Page 28460, line 16: How H₂O increase will promote formation of NAT particles? For ICE particles it can be real, but they are not included.

6. Page 28460, line 16-18: This statement should be supported by a proper reference. I do not think the sentence is completely correct.

7. Page 28460, line 19-21: This sentence should be extended to give the readers an impression how it can be done.

8. Reference list: In my version there some extra numbers (they look like page numbers) at the end of references.

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9. Figures: why not to show ClOx for 2000-2010?

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 28451, 2012.

ACPD

12, C10782–C10785,
2012

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