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Atmospheric Chemistry and Physics Discussions

Interactive comment on "A closer look at Arctic ozone loss and polar stratospheric clouds" *by* N. R. P. Harris et al.

Anonymous Referee #2

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Review of acp-2009-878 manuscript "A closer look at Arctic ozone loss and polar stratospheric clouds" By N. R. P. Harris et al.

This article is the follow up of a previous study published in the International Journal of Remote Sensing. Both studies intend to provide an explanation for the compact relationship between the integrated ozone loss in the Arctic and the volume of polar stratospheric clouds as observed in the recent Arctic winters. The article is well written and suitable for publication in Atmospheric Chemistry and Physics. However the discussions are mainly qualitative and major revision is needed to improve the quality of the manuscript, as described below:

Major comments

1. The present study should distance itself from the previous one. Some figures (e.g. figures 4 and 7 are very similar to figure 2 and 3 of the previous article). Some discussions (e.g. activation and ozone loss period) are also very close. The authors should build on the previous study and emphasize the new results.

2. The main incentive for the study is the compact relationship between integrated ozone loss and the volume of PSC as represented in figure 2. However, the error bars in the figure are barely explained nor is evaluated the effect of these error bars on the sensitivity slope of ozone loss as a function of cooling. A value of 15 DU per 1° cooling is provided without error bars. If the error bars are large, could it affect the usefulness of this relationship in climate models?

3. The discussion is very qualitative and sometimes fuzzy as detailed below. An explanation based on the factors that mainly affect the temporal evolution of ozone and other major constituents, as expressed by the continuity equation would give more weight to the demonstration. Sensitivity figures demonstrating cancelling effects of e.g. the ozone loss and deactivation would be useful.

4. The whole methodology is based on a photochemical box model run along idealized trajectories. How these trajectories mimic the real atmosphere should be better demonstrated in the article. Some sensitivity studies (e.g. denitrification) should also described in the context of observations.

Detailed Comments

1. 6683, l13: Bodeker et al., 2005 should not be the only reference for the evaluation of Antarctic ozone loss in 2002. More references should be provided on this rather well documented event. 6683, l19: the sentence on the capacity of 3D models to reproduce ozone loss is too vague.

2. Figure 1: how is the vortex defined? The definition of the vortex should be clearly stated in the article.

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3. 6684, l8: PSC “fall” should be replaced by “sediment”.

4. 6685, l15: Figure 2 that is introduced here needs a detailed explanation on how the various quantities and their error bars are derived. An explanation is provided later in the methodology section but the organization of the article should be revised so that the figure and the various terms are adequately described, including the sensitivity relationship of ozone loss versus temperature.

5. 6686, l15. Provide reference for the assertion that PSC are unlikely to form above 24 km.

6. 6686, l20-25 & discussion on PSC page 6688-6689: The discussion on Vpsc computation is somewhat fuzzy. Could the various assumptions on HNO3 and H2O mixing ratio translate into error bars in Vpsc? It is also not clear how the estimate in the article compare with estimations from CALIPSO. A much more focused discussion is needed here.

7. 6687, l11-18: Provide explanation for the design of the idealized trajectories. What is the reason for the sinusoidal 6 day cycle of 20°?

8. 6688, l5: how good are the diabatic heating rates calculated in the SLIMCAT model, e.g. how do they compare with N2O isopleths?

9. Figure 3: The description of this figure is quite fuzzy. (1) There is a typo on the O3 loss units (it should read 1012 mol cm-3. Further, in order to show the contribution of each layer to the loss, I suggest to use DU/km. (2) Why is there no error bar in the figure as in figure 2? What are the quantitative values of the slopes and “scatter” at the various levels? Is it pertinent to relate the Apsc value and ozone loss at the same isentropic level, since air masses can be in the presence of PSCs at a certain level in the beginning of the winter and then descend to another level due to diabatic heating at the end of the winter?

10. 6690, l1: A summary of the main heterogeneous and homogeneous chemical re-

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actions playing a key role is needed here. These reactions could then be used to derive quantitative relationships for the temporal evolution of ozone and other key compounds (e.g. ClOx, HNO3). In addition bromine compounds are also barely mentioned in the article. How the uncertainties on the effective BrO levels in the lower stratosphere affect the O3 loss/ Vpsc relationship?

11. Discussions p. 6691& 6692: as emphasized previously the discussion on the cancelling effect of ozone loss and deactivation would greatly benefit from quantitative relationships.

12. 6694, l14-17: The counteracting effects should be quantified. The adjective “small” is very vague.

13. 6695, l15: To what extent the sensitivity study on denitrification correspond to denitrification observed in Arctic winters? Reference should be provided.

14. 6697, l15: The approaches for parameterization of Arctic ozone loss according to the methodology developed in the article should be developed in a more detailed way, e.g. what would be the advantage of the second approach with respect to the first one? An evaluation of the temperature range for which the simple “scaled” relationship can hold would also be very useful.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 6681, 2010.