

***Interactive comment on* “Temporal evolution of chlorine and minor species related to ozone depletion observed with ground-based FTIR at Syowa Station, Antarctica and satellites during austral fall to spring in 2007 and 2011” by H. Nakajima et al.**

Anonymous Referee #2

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The manuscript by Nakajima et al. focuses on ground based FTIR observations of trace species in 18 and 22 km altitude for the years 2007 and 2011 made from the Japanese Syowa station on Antarctica. These data are compared with co-located MLS data and monthly average ozone sonde data. The manuscript further shows simulations with the chemistry climate model MIROC3.2 nudged to ERA-Interim data for the observation years 2007 and 2011.

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Although there are likely no severe issues with the data or the simulations, the scientific goal of the manuscript remains completely unclear. I find that it is not sufficient, just to present the observations and simulations without addressing open scientific questions. The authors present no new novel concepts, ideas, or tools. The only thing that is said that they present the first ground based continuous measurements of chlorine species in Antarctica.

The model data are displayed in addition to the observations but not on the same altitude levels and they are not used to interpret the observations. There is no obvious connection between the simulations and the observations besides two appendix figures that contain the time series of the chlorine compounds and ozone over Syowa station from model and observations.

With respect to the interpretation of the data, it seems that only few aspects of the observations and the simulations are mentioned. Most of them are in line of what is expected and shown in standard chemistry model runs over the last two decades. The simulations seem to be in line with the observations. But it remains completely unclear, what the message of the paper is.

Furthermore, there are many small inaccuracies in the text, some of them are summarised below. Also, many informations are not given precisely, such that the reviewer needs to guess, what the authors meant. Because of the missing scientific concept, I would not recommend the paper for publication in Atmos. Chem. Phys.

General

The introductions lists some textbook knowledge but it is not clear, why it is at all important in the context of the manuscript.

The use of ClO data as done in this paper is problematic, since one needs to take into

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account the diurnal cycle typically involving Cl_2O_2 as a nighttime reservoir. Observations (fig 6-12) are likely for different local times and are therefore not comparable. As in the 3-hourly model data of fig 17, no diurnal cycle is visible, it is likely a zonal average over daytime and nighttime data. That is not useful for comparison. Potentially this is the cause of the ClO difference in figs B1 and B2.

Figures 13-16 show the model output on the 50 hPa level for 4 different times. It is not so clear, what can be learned from these figures. Also it would be better to not use a different vertical coordinate (pressure) for the model results as opposed to the observations (altitude).

fig 10/11 shows Cl_y (MLS) relative to Cl_y^* (MLS N_2O), even though that should be described more clearly. In the context Cl_y (without star) is defined as ClO + ClONO_2 + HCl. However, there are no ClONO_2 observations of MLS. This needs to be clarified.

Details

page 1/line 16 "is not well understood": I don't think that this statement is justified.

1/22 PSC saturation temperature: you likely mean "PSC existence temperature".

2/3: "from active chlorine" or "from ClO" ?

2/17: the expression "inert chlorines" for HCl and ClONO_2 is not typical, please use the wording "chlorine reservoirs" (as in 3/31).

3/4-14: The chlorine deactivation into ClONO_2 or HCl is mentioned, but not that it depends on ozone (Douglass et al. 1997, Groöb et al., 2005 JAS, Groöb et al., 2011 etc;).

3/19: the phrase "super-recovery" is not ideal. It is sometimes used for ozone but not often for ClONO_2 .

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3/22: ozone has been monitored before the discovery of the ozone hole. (otherwise the ozone hole would not have been discovered).

4/18: "analysis" Do you mean retrieval of tracer profiles from the FTIR spectra?

4/22: how many layers exactly?

5/12: As you only show 14 coincident measurements within a period of 3 months, I would not call this chapter "Validation".

5/31: You show results from MLS Version 3.3 data. Why do you not use version 4.x?

6/8: How exactly do you identify the coincident CALIPSO PSCs? Orbit within a certain distance from Syowa station? PSCs of what type at what altitude?

6/12-15: This seems to be a speculation. It is not clear how this statement is proven in this context.

6/16: You use the term "temporal variations" several times, where I think it is (only) a time series.

6/16: define the expression Cl_y^* .

6/17: add "for all ground-based and satellite based observations used in this study" (or similar).

8/5 ratios of each species with respect to Cl_y^* .

9/1ff: It has not been said over what time the data were collected, how the anti-correlation was evaluated (MIPAS and MLS have different orbits).

page 9/line 1-6 (anti-correlation of MIPAS $ClONO_2$ and MLS ClO , fig 12): If is said that this is due to the PV (eq. latitude) dependence. Could it also be that this occurs because of the time dependence of the deactivation throughout the days 220-260? The slope of the regression line is not given in the text nor a statement of what would be expected from the model. What does this slope or correlation mean scientifically?

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This correlation in the phase of chlorine deactivation is definitely no surprise.

9/24 "About 50% This statement is inaccurate (do you mean at the 50 hPa level equally from the vortex edge to the core? Or also at the other levels? It is at least rather hard to read a number of percentage ozone loss from this figure.

9/33f "Inside the polar vortex, depletion of O_3 , NO_2 , HNO_3 , and $ClONO_2$ continued" Most of the species are already near zero. It is not clear from the figure how you see continuing depletion. Also I would not expect further ozone depletion, if active chlorine returned more or less to zero.

10/23-28: The continuous loss of HCl seems to look differently than in the study by Grooß et al. that you mention. The conclusions of that study are not given properly. It is not clear, whether you include an additional process like ionisation by cosmic rays or cross vortex edge $ClONO_2$ flow due to Solomon et al. in your model. Or does the HCl just deplete because of the large diffusivity that is present due to the low model resolution (2.8×2.8 degrees)?

11/18f: I do not see this good agreement between model and FTIR in the figures.

figure 5: How was T_{NAT} and T_{ice} derived? What data for HNO_3 and H_2O were used?

figure 12: There must be something wrong with the colour coding of the PV in the panels. I would expect about a factor 2 difference in PV between 18 and 22 km and also PV values significantly below -85 PVU at 22 km.

figure 17: "Three-hourly zonal-mean temporal variations" What do you mean by variations? It only looks like zonal mean values.

figures B1/B2: Here the model is labelled CTM. Is it really? In the paper you always talk about a CCM.

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Typos/grammar

6/12: other reason -> "other reasons" or "an other reason"

8/32 shows

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