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Atmospheric Chemistry and Physics Discussions

# *Interactive comment on* "Polar Stratospheric Cloud evolution and chlorine activation measured by CALIPSO and MLS, and modelled by ATLAS" *by* H. Nakajima et al.

**Anonymous Referee #1**

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General comments

This study by Nakajima et al. focuses on the understanding of the PSC composition and chlorine activation according to the temperature evolution of the air masses. A lagrangian approach is used for this purpose with the use of satellite data: CALIOP for PSC composition and MLS for chemical species concentration, and ATLAS model simulations. The study provides some interesting results. First, it corroborates previous studies indicating that NAT particles could form without the presence of ice core. Second, it shows trough some various examples that chlorine activation is limited first by the ClONO2 amount and does not depend on the PSC composition in case of temper-



atures staying long time enough below TNAT-4K (∼1-2 days). In case of temperatures short time below TNAT-4K, the chlorine activation is not limited by the ClONO2 amount and is very dependent on the temperature accuracy. The manuscript is globally well-organized and well-written. I only have some comments detailed below.

Major comments

1) Page 22160, line 26 the authors mention that "the "STS+NAT" and "CALIPSO constrained" runs were closer to the measurements than the "STS"run." According to the legend in figure 8, "STS+NAT" is in black, "CALIPSO constrained" in blue and "STS" in grey. Thus the "STS+NAT" and "STS" runs are closer to the measurements than the "CALIPSO constrained" run. This impacts the conclusion given between page 22160 line 29 and page 22161 line 1. The authors cannot claim that the amount of chlorine activation on PSCs is dependent on PSC classification. Same remark for page 22162 line 24 and in the abstract page 22142 line 20. However, this remark are not such as to call into question the main results of the paper but should be rectified.

2) For "STS+NAT" run, the authors use a NAT particle number density of 0.1cm-3 while for the "CALIOP constrained" run, they use 10-3 cm-3. In a same manner, they use an ice particle number density of 0.01cm-3 while for the "CALIOP constrained" run, they use 1 cm-3. In order to better compare the results of the two runs, why do the authors not use the same particle number densities?

3) The first part of the paper discusses the link between temperature evolution and CALIOP PSC composition observation. However, there is no discussion/explanation of why temperatures are sometimes below the PSC thresholds and no PSC are observed by CALIOP. This is the case in figure 4 (day 4), figure 5a (day 2), figure 5b (day 2), figure 11 (day 2), and figure 13 (day 4). This also concerns the second part of the manuscript for cases #02 (day 2) and #09 (day 4 morning) where PSC are simulated by the ATLAS model but not observed by CALIOP. Some comments should be included in the manuscript to explain these differences.

Minor comments

1) Page 22146, lines 4-9. Could the authors specify the maximum time difference between CALIOP measurements and the trajectory points? For the distance, it is less than 100-200 km?

2) Figure 2. Could the authors better explain how they get the CALIOP field? As CALIOP data are only available where circles are, how the PSC composition between these circles is determined? The method detailed page 22147 to get the PSC field is not clear.

3) In this study, the authors use the MLS version 3.3. The MLS version 4.2. is now available but for HCl and O3, it seems that there are very little or no change compared to V3.3/V3.4 according to the "version 4.2x Level 2 data quality and description document" (JPL D-33509 Rev. A). Page 22146, line 22, the authors can mention that they also use H2O data as explained page 22153 line 6.

4) Page 22147 line 14, the authors should change "below TNAT" to "above TNAT".

5) Page 22150, lines 3-5, could the authors give a reference for this value of supersaturation needed?

6) Page 22150, line 26, could the authors indicate the value of the assumed supersaturation for HNO3 over NAT? Is it 10 as mentioned before?

7) Page 22155, line 23, to help the reader, the authors should add the reactions :

ClONO2+HCl–>HNO3+Cl2

ClNO2+H2O–>HNO3+HOCl

HOCl+H2O–>Cl2+H2O

When the authors mention from page 22143 line 28 to page 22144 line 3 the reaction partner, I suppose that they refer to these reactions.



8) Page 22156, lines 3-5, could the authors indicate why the ATLAS model needs larger spatial and time scales to reproduce HNO3 measurements?

9) For figures 8 to 14, it is necessary to use a 400 % zoom in order to read the figures and see the differences between black, grey and blue curves. The authors could probably only focus on the part between days -1 and days 5. The figures are very well described in the legend. However, explanation about the short backward trajectories observed on figures (d) to (i) is missing. I assume that these are the short back trajectories done to find the last model output of the global model for chemical initialization. Likewise, on figures 8 to 14 (h), there are dotted lines not explained in the legend. I expect it represents the HNO3 total (gas phase and condensed).

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