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

H. Nakajima et al.

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We would like to thank Referee #1 for his/her time, constructive and helpful comments and suggestions.

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

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“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.

You are right. We agree that we cannot claim that the amount of chlorine activation on PSCs is dependent on PSC classification. Also, there is inconsistency between PSC number densities used for “CALIOP constrained” run and other (“STS+NAT” and “STS”) runs, as is pointed out in the following comment 2). This inconsistency comes from different PSC models used for these runs. We discussed on this issue among co-authors, and concluded that it is no use to show these three model run results in the paper, because there is no fundamental differences in the model results to discuss. Therefore, we decided to show only “STS+NAT” model run results throughout the paper.

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 1cm^{-3} . In order to better compare the results of the two runs, why do the authors not use the same particle number densities?

As is stated in the above reply for your comment 1), this difference comes from the different origin of the PSC models. As is stated above, we decided to show only “STS+NAT” model run results throughout the paper.

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

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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.

There are several possible reasons for this: A) We have to interpolate the CALIOP PSC measurements to our trajectory locations, which introduces uncertainty, since we don't really know what type of PSC is on the location of the trajectory. B) Uncertainties in the CALIOP measurements and classification. C) Uncertainties in ECMWF temperature. D) Another possible reason is that this could be real: PSC formation is a complicated process that depends e.g. on temperature history. A) is actually discussed in detail in the paper (page 22146, 4-15). B) is discussed in other papers (Pitts et al., 2007, 2009, 2011). C) is also discussed to some extent in the text. The contents of D) is out of focus of this paper.

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?

The maximum time difference is 24 hours. We have added the description 'closest to each trajectory point "on the same day"' on page 22146, line 5. You are correct that the distance is typically around 100-200 km. We hope that is implicitly clear from the statement on page 22146, line 14-15.

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.

We have added the description "Global PSC field was created by selecting the closest CALIOP PSC measurement on the same day." at the end of page 22147, line 18.

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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.

At the time when the calculations were made, no version 4.2 data was available. We added H2O as a species on page 22146, line 22.

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

“below” is correct here. It refers to “period” and not to “sudden stratospheric warming”.

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

We have added references.

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?

This is the general model description, and the value for the supersaturation is not fixed in the model. Hence, the value is only given in section 4.2, where the actual model setup for the specific runs is described.

7) Page 22155, line 23, to help the reader, the authors should add the reactions : $\text{ClONO}_2 + \text{HCl} \rightarrow \text{HNO}_3 + \text{Cl}_2$ $\text{ClONO}_2 + \text{H}_2\text{O} \rightarrow \text{HNO}_3 + \text{HOCl}$ $\text{HOCl} + \text{H}_2\text{O} \rightarrow \text{Cl}_2 + \text{H}_2\text{O}$ 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.

Thank you for the suggestion. We added these reactions on page 22143, and showed the number of reaction (1) on page 22155.

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

Since the discussion on O₃ and HNO₃ is not essential part of this paper, we deleted all the discussion on O₃ and HNO₃, and related panels from the paper completely.

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).

We need the time axis from -5 to +5 days, since the trajectory is initialized by the MLS measurement of the last 5 days which is described in section 4.3, and these are shown in the plot. The all O₃ and HNO₃ panels are deleted from the paper (see reply to your comment 8).

Please also note the supplement to this comment:

<http://www.atmos-chem-phys-discuss.net/15/C12137/2016/acpd-15-C12137-2016-supplement.pdf>

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 22141, 2015.

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