Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2019-443-RC2, 2019

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Chlorine partitioning near the polar vortex boundary observed with ground-based FTIR and satellites at Syowa Station, Antarctica in 2007 and 2011" *by* Hideaki Nakajima et al.

Anonymous Referee #1

Received and published: 3 July 2019

This paper uses ground-based FTIR measurements of stratospheric trace gases at the Syowa Station, as well as satellite and model data to investigate relationships between chlorine species during spring-time ozone depletion. A variety of interesting datasets are presented, but it is difficult to follow the arguments that the authors are putting forward because of how the paper is organized and how figures are presented. Overall, I think that the paper could make a good contribution to ACP after major revisions, as suggested below.

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GENERAL COMMENTS

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More context for the FTIR data should be given. A few questions come to mind.

- Is this the first time that the Syowa FTIR data has been retrieved/written up? Or are there other papers that describe this instrument and the data?

- Why is there only 2007 and 2011 data? Was the instrument not deployed in other years?

- Are the data publicly available?

- How to these data fit in with other ground-based FTIR datasets collected at high latitudes? Is the data quality similar? Have other FTIR instruments been used to study trace gases during spring-time ozone depletion?

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The validation sections should be more specific in terms of how validation methods are applied (see specific comments for details). It would also be useful to answer:

- What is the expected uncertainty in the FTIR measurements? Is this something you retrieve?

- How do your validation results compare with validation of other ground-based FTIR instruments? Is the instrument at Syowa performing similarly to other FTIR?

- Are there any other factors that could affect comparison results? Do any of these species vary diurnally? Is there a chance that the satellite and station are measuring very difference air masses (e.g., inside/outside the vortex) for some coincidences? If either of these are factors, how does this affect interpretation of the data in Sect. 4?

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The discussion of results is repetitive and at times is difficult to follow. There are many

timeseries figures (Fig. 4-9; Fig. 11-12). I found it hard to track through these and determine what exactly the authors were trying to highlight. I also had a hard time connecting all of these pieces to the concluding arguments around transport, chlorine deactivation, etc. A few possible solutions:

- Determine what you are trying to show, perhaps starting from the statements made in the conclusion. Only include figures/discussion that are relevant to what you're trying to show. Move additional timeseries figures into an appendix or supplement. Some tables (Table 1, Table 4) could also be moved out of the main body of the paper.

- For Fig. 8-9 is there a more concise way of showing the relationships between the chlorine species than using the timeseries (e.g., through scatter plots or something else)?

- Can the comparison with the modelling data be merged in with the validation of the FTIR (Sect. 3)? Can the modelling timeseries figures be folded into the timeseries figures presented in previous sections?

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I found it hard to follow the discussion around the mechanisms behind the decrease of HCl. It would be helpful if the discussion was expanded, to help answer the following:

- What hypotheses are there for the decrease in HCl in the literature? Is there just Solomon et al. (2015) and Grooss et al. (2018)? Are these hypotheses in conflict with each other or is it possible that they both contribute to the decrease in HCl together?

- For the existing hypotheses, what data/evidence were used to develop the hypotheses? How does the data that you collected add to the existing supporting data/evidence?

- Why does the sporadic increases in ClONO2 in the model data support the transport mechanism? Do you have any other data to support that ClONO2 is being transported (e.g., maps of ClONO2, tracers showing transport patterns, evidence in the satellite

data)? Are there alternative mechanisms that could explain your observations, such as chemistry?

- Can your data be used to refute any other hypotheses for decreasing HCl?

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SPECIFIC COMMENTS:

- Page 1, Line 17: You state “This was the first continuous measurements of chlorine species throughout the ozone hole period from the ground in Antarctica” here and elsewhere in the paper. This statement is a bit vague. Have other studies looked at any chlorine species from the ground in Antarctica? Are they looking at fewer species? Or for shorter time periods? It would be helpful if you included a literature review of ground-based measurements (FTIR and maybe other measurements of chlorine, such as OClO from UV-vis?) in Antarctica in your introduction.

- Page 4, Line 25: Are there other ground-based FTIRs in Antarctica?

- Page 5, Line 1: What is the temporal resolution of the FTIR at Syowa? Does it only take measurements during sunny days? Does it require manual operation?

- Page 5, Line 18: Is there a way of using the averaging kernel to determine whether the 18 km and 22 km concentrations are independent from each other?

- Page 5, Line 25: State where various satellite datasets were obtained from? (E.g., URL?)

- Page 5, Line 24: The application of selection criteria should be clarified, perhaps with a full paragraph at the end of this section. Was this same criteria applied to all three satellite instruments (MLS, MIPAS, and CALIPSO)? Was the same criteria used for both the validation (Sect. 3) and the discussions of results over Syowa (Sect. 4)? E.g., for the timeseries figures was the 6 h criterion applied?

- Page 5, Line 30: Have you considered applying a selection criteria based on in-

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side/outside the polar vortex to the satellite data along their line of sight instead of using values over the station for all measurements?

- Page 6, Line 18: What is the shape of the 5 km-wide slit function? Is this based on the FTIR resolution? Why was this used instead of the averaging kernel? Also, why are the MLS data smoothed? Is the vertical resolution for MLS much higher than the ground-based FTIR?

- Page 6, Line 25: State the expected uncertainty and any expected biases in ozonesonde measurements

- Page 6, Line 24: I'm a bit confused about the language used throughout this section. The figures show mean relative difference and mean absolute difference versus altitude. Replace "The absolute difference. . ." with "The mean absolute difference. . ." and "The mean relative difference" in this line.

- Page 6, Line 25: What do you mean by "mean relative difference" here? Is this the mean relative differences averaged again over an altitude range? It might be simpler just to state the mean relative difference values at 18 km and 22 km instead of taking another average over altitude.

- Page 6, Line 26: What to you mean by "within error bars"? Are you referring to the standard deviation in the mean differences? This isn't really an error – it's the variability in the comparisons. It would make more sense to determine whether the agreement is as expected based on estimated uncertainty in the FTIR measurements (if this exists) and known uncertainties/biases in the satellite data. Also, why is the standard deviation used for the comparisons instead of the standard error?

- Page 6, Line 31 – Page 7, Line 2: See comments for previous paragraph.

- Page 7, Line 10: "with a precision of 0.2-0.6%" – is this the precision in the MLS measurements? Or is this the precision of the systematic bias?

- Page 8, Line 6: You describe a set of steps for detecting the inner and outer edges of

the vortex. Is this a new method? Is this expected to work better than other established methods for some cases?

- Page 8, Lines 11-14: Please break this down into smaller steps – I have a hard time understand what was done. My best guess is that you calculated the isentropic potential vorticity gradient as a function of equivalent latitude. You found the local maxima. You defined the inner/outer edges of the vortex at the local maxima that both (?) exceeded the wind-speed threshold and were at least 5° in equivalent latitude apart? Are there always only two local maxima that meet these criteria?

- Page 8, Line 18: Did you filter the MLS data according to an error threshold? Or did you just remove suspect data when you looked at the timeseries? All filters applied to the satellite data (for, e.g., uncertainty, etc) should be described explicitly in Sect. 2.2.

- Page 11, Lines 10-24: Can the model comparisons be merged into Sect. 3? It would be nice if consistent comparison methods were applied. Similar to Sect. 3 – are the comparisons consistent with what is expected based on known model performance and known biases in the satellite instruments?

- Page 11, Line 28: Why was a different method used to determine the polar vortex than at Syowa station?

- Page 11, Line 31: What do you mean by “This boundary was located in between the inner and the outer edge of the polar vortex as were defined in Sect. 4.1”? Did you compare the two definitions of the vortex? If so, was this just done at Syowa station for all data or a subset of data? Or is it the case that the max gradient of PV at 475 K is always between the inner/outer edge values by definition?

- Table 4: Should describe how you came up with each of the various ranges presented in the table. Here are a few examples of questions that should be answered. How do you define the threshold for a ClONO2 enhancement or ozone starting-ending day of decrease? What are given in “HCl Value after increase” ranges? Is this the min/max

of individual FTIR measurements over a given time-period? For the ClO enhancement period, is the 80% of maximum value different in 2007 and 2011 or is a single value used? What does "Variation when HCl ∼0 ppbv" mean?

- Figure 3: Can you add a panel describing hours of sunlight or SZA here or to one of your other timeseries? It would be helpful to visualize this through discussions around available sunlight.

- Figure 3: Are the gaps in the PSC timeseries because CALIPSO observed no PSCs or because CALIPSO didn't collect any coincident data during this time? Either describe in text and/or add marker for CALIPSO measurements which did not observe PSCs.

- Figures 14/15: Why have you used model data instead of satellite data for the parameters that are available from satellite? Did you check to see if the satellite saw the same patterns as the model?

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MINOR/TECHNICAL COMMENTS:

- Throughout the text, there are some awkwardly worded sentences and minor grammatical mistakes. I assume that these would be corrected through copy-editing, so have not listed these here, unless the meaning is unclear.

- Table 2: Define "PT"

- Table 3: Should define the fields included in the table. Is D(%) 18-22 km the average of the mean relative differences? Is (Min/Max %) 18-22 km the max/min mean relative difference across the various altitudes? (Might be better just to show mean relative differences +/- standard error at 18 km and at 22 km.)

- Table 3: See comment re: precision in Page 7, Line 10

- Table 3: Replace "Agreement" with "Range of mean absolute differences for 15-25

km”

- Figure 2, caption: Replace “Absolute (a) and percentage” with “Mean absolute (a) and mean percentage”

- Figure 2, caption: Replace “FTIR measurements and those from ozonesonde” with “FTIR measurements minus those from ozonesonde”

- Figure 2, caption: Move “Horizontal bars indicate the standard deviation of differences at each altitude.” to the end of the caption, since it applies to all panels.

- Figure 2, caption: Describe what the horizontal dashed lines indicate.

- Figure 3, caption: Replace “from N2O value” with “from Aura/MLS N2O”.

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