

## Response for the Review (acpd-14-c1555-2014)

Kazutoshi Sagi

Dear Reviewer,

Thank you very much for your comments on our paper. We will introduce all suggested corrections. Below we present responses to your specific comments and questions.

### Page 7890

#### Reviewer:

Lines 2-3: what is this high “sensitivity” (vertical or horizontal or any other)?

#### Author

>> High sensitivity means here “low noise” and thus SMILES has high precision and good measurement response. We have changed the text in the manuscript.

#### Reviewer:

Line 3: “high latitudes” ? write the exact latitude range

#### Author

>> We have added the exact range.

#### Reviewer:

Line 6: “ozone loss due to the instability of the polar vortex”? What ozone loss is this?

#### Author

>> This text is wrong. The original intension was to attempt to explain that due to its instability the vortex was measured by SMILES despite the instrument’s latitude limitation. We removed the sentence.

**Reviewer:**

Line 11: Only wind data? If you have also used temperature data, then use “meteorological data are taken from ...”

**Author**

>> We only used wind data.

**Reviewer:**

Line 13: I am confused. What is your focus, “cross-isentropic transport” or ozone loss in 2009/10?

**Author**

>> The first part of the paper focuses on implementing a vertical transport scheme to account for descent inside the vortex into the DIAMOND model. We then used the modified DIAMOND model for the analysis of ozone loss in the 09/10 winter.

**Reviewer:**

Line 14: Limited latitude coverage of SMILES or SMR? and what is that coverage (lat. to lat.)

**Author**

>> Limited coverage of SMILES observation. Specific latitudes were written in line 4.

**Reviewer:**

Lines 16—18: What is this “significant” ozone loss? How is it defined? You also need to say the exact altitude and period of the ozone loss.

**Author**

>> This is explained in the following statement.

**Reviewer:**

Line 18: The ozone loss started by the end of January with 0.6—1.0 ppmv? No. your analysis itself shows ozone loss even in late December. Please look at your figure 8.

**Author**

>> The statement in this line is for the first loss that occurred below 500K. Actually we also found small losses in late December at around 700K in SMILES data (figure 8b), which was not mentioned in the manuscript. Please check my response about this loss to the comment from anonymous referee #2. We have added a new explanation in revised paper.

**Reviewer:**

Lines 19—20: "... loss started ...". This contradicts your previous statement (which was also not correct). Also, I do not see these distinct phases of ozone loss in Figure 8.

**Author**

>> The previous statement at line 18 gives the explanation of the loss below 500K caused by the PSC formation and reactive chlorine species. On the other hand, text at lines 19—20 is written for the ozone loss at around 600K which is likely induced by NO<sub>x</sub> related chemistry.

**Reviewer:**

Line 21: Lower stratosphere? No, not all altitudes. Specify the altitude range for this ozone loss.

**Author**

>> We have added the altitude range.

**Reviewer:**

Lines 23—24: This is common to both polar regions, not only for Antarctica, as you have mentioned in *Conclusions*. Please rewrite the sentence.

**Author**

>> We agree. We simply removed "over the Antarctica" from the sentence.

**P7891**

**Reviewer:**

Line 1: Wave activity is relatively higher in the Arctic.

**Author**

>> Yes, we agree.

**Reviewer:**

Lines 3—4: Which fact? PSC formation / propagation of planetary waves / vortex instability? Please reformulate this sentence.

**Author**

>> We agree that this was not clear. We changed the word “fact” to “effects”.

**Reviewer:**

Line 5: No, this is not correct. You need to specify the altitude and period to make such a strong statement.

**Author**

>> We have modified the text.

**Reviewer:**

Line 7: 600 K is a bit higher for Arctic ozone loss analysis. It would be better to discuss the temperature structure at 450/475/500 K. Also, please refer Dornbrack et al. (2012) and discuss their findings with respect to your temperature analysis.

Figure 1: 600 K is very high for Arctic ozone loss analysis. What is the motivation for selecting this altitude? At 450 or 475 K would have been more useful for this study. I also do not see that you discuss the ozone loss at 600 K in that detail with respect to your temperature analysis.

**Author**

>> We fully agree. 600K is not a good choice for the ozone loss analysis. However it is difficult to get reasonable results below 475K from SMR ozone. So we select the potential temperature of 500K to reproduce the figure. We referred to Dornbrack 2012.

**Reviewer:**

Line 10: Not completely true for all PSC types, as it also depends on temperature.

**Author**

>> Sorry but we don't understand this comment.

**Reviewer:**

Lines 12—13: As you haven't done the analysis of major warming of this winter, you need to cite a publication in which this analysis is given (e.g. Kuttippurath and Nikulin, 2012).

**Author**

>> We refer now to the suggested article.

**Reviewer:**

Lines 15—16: The instability of the vortex was not due to the warmings? On the other hand, you have already stated that the temperatures were as low as 180 K in early January.

**Author**

>> Exact. SSW constrains the instability of the vortex. We have changed the statement.

**Reviewer:**

Line 24: Latitude range 38 N or 38 S. You need to state that clearly (somewhere earlier in this section, i.e. your measurements were during this particular period/winter and therefore, you calculate ozone loss for that winter). Otherwise, the readers might ask why you selected this winter for your study.

**Author**

>> Sorry again for the wrong latitude range of SMILES observations. We corrected the latitude range.

**Reviewer:**

Line 23: The instrument operated only for this short period?

**Author**

>> Unfortunately yes. SMILES stopped the observation because the local oscillator broke down at the beginning of April.

**Reviewer:**

Line 28: Not because of its latitude coverage, but due to its limited coverage in the high latitudes. Also state the latitude band of those measurements (e.g. 38N—65N)

**Author**

>> We agree and have changed the wording.

**Reviewer:**

Line 29: Why do you want to compare with SMR, as there are other satellite measurements available for this winter with better altitude and latitude coverage (e.g. Aura MLS)? Perhaps, you would like to compare measurements from similar instruments?

**Author**

>> As you guessed, similarity of the instruments is one of the reasons. Another point is that Odin/SMR started its observation from 2001. We have several studies of ozone loss in different winters based on SMR data and the original version of DIAMOND. That is why we were familiar with SMR ozone data. Since you and the other reviewer suggested it, we decided to have an additional section to compare with other studies including MLS measurements.

**Page 7892**

**Reviewer:**

Line 1: Dynamical instability permitted more measurements? I did not understand this.

**Author**

>> Yes, because the unstable vortex sometime moves toward lower latitudes. We fixed the text.

**Reviewer:**

Lines 5—6: missing or not possible? Line 7: blocked at “high latitudes” or blocked “high latitude measurements”?

**Author**

>> Not possible. SMILES could not measure at that time because of the ISS. We have modified the text.

**Reviewer:**

Line 20: “Since it is a two-dimensional model, ...” Start the sentence something like this.

**Author**

>> Done

**Reviewer:**

Lines 20—21: Is this sentence complete?

**Author**

>> yes

**Reviewer:**

Lines 23—24: State why SMR is used for your comparison, if not mentioned before.

**Author**

>> We added the statement.

**Page 7893**

**Reviewer:**

Line 2: Why did you use N<sub>2</sub>O in this study? Please write the reasons for this (e.g. Checking dynamics in the model).

**Author**

>> Yes, we used N<sub>2</sub>O for checking dynamics in the model because of its long lifetime. However the reason is written at lines 25—27 in P7890, we have modified the text to be clearer.

**Reviewer:**

Lines 23—24: Please add a figure of Averaging Kernels (with FWHM), which would give an idea of the vertical resolution of these SMILES retrievals. Please make sure that you select a retrieval at around 60 N.

**Author**

>> We don't add the Averaging Kernels in the manuscript because it has already presented in Kasai et al. 2013. However, we have rearranged the text. We selected all measurements which have measurement response greater than 0.85 for the data assimilation analysis.

**Reviewer:**

Lines 25—27: Any validation results above 50 N to mention here? That would be more useful for this study. Also, what is the latitude range for this “mid-latitude”, 30—60 N/S?

**Author**

>> The description of the latitude range in this line is not correct. The SMILES ozone from 65S to 65N was selected for validation with SMR (Kasai et al. 2013). We have corrected this in the revised article.

**Page 7894**

**Reviewer:**

Lines 2—3: Which band is used in this study (A or B)?

**Author**

>> We used ozone data from both band A and B because there is no significant bias between them. Most ozone data was from band B and band A ozone was used when band B ozone was not available.

**Reviewer:**

Line 9: What is LST, Local Solar Time? If yes, please write that.

**Author**

>> Yes, we corrected this.

**Reviewer:**



Line 24: 60 N or S?

**Author**

>> 60N-90N. Corrected.

**Page 7895**

**Reviewer:**

Lines 1—2: SMR N<sub>2</sub>O has vertical resolution of 1.5 km?

**Author**

>> Yes

**Reviewer:**

Lines 3—7: “Other measurement comparisons.....”. If the listed studies/comparisons provide some information on the accuracy of SMR N<sub>2</sub>O, then please write that explicitly here. Otherwise remove this sentence.

**Author**

>> We have added specific information of the validation from listed studies.

**Reviewer:**

Line 18: “potential temperature levels ranging from...”

Line 24: Yes, it is conserved. But, please mention the duration with respect to altitude.

**Author**

>> Corrected.

**Page 7896**

**Reviewer:**

Line 4: “To account for this”, to account for diabatic descent?

Lines 5—7: Please reformulate this sentence.

Line 11: It is also appropriate for you to give the equation of advection, as you discuss this term later in Results section too.

Line 16: Speed of the phenomenon? What phenomenon?

Line 23: Remove theta, as you have already defined this in Line 7.

**Author**

Line4: Yes.

Line 5-7: We have changed the statement.

Line 11: We have changed the equation.

Line 16: The phenomenon here is the diabatic descent inside the vortex.

Line 23: We have removed theta.

## **Page 7897**

### **Reviewer:**

Line 4: Delete “fields”

Line 6: What is your investigation period, December to March? Please mention that too.

Line 8: What is this measurement response? Derived from averaging kernels/FWHM?

Lines 14—15: “In the results, we..”, Is it because of the altitude coverage of the measurements or any other reason?

Line 17: write “US (United States) ...”.

Lines 23—25: Please mention the exact time period or dates; “at the beginning of the winter”, “one or two weeks ...” are not enough. Also, Kuttippurath and Nikulin (2012) have given a detailed analysis of these processes with potential vorticity maps. Please mention their findings here.

Lines 26—28: You need to cite Kuttippurath and Nikulin (2012) too here, as the central date (this is 9 February 2010 in their analysis) depends on the data used for the analyses. In addition, a detailed study of the major warming of this winter is presented by these authors.

Line 28: Cite Dornbrack et al., 2012 here, as they have given a detailed analysis on temperatures of different Arctic winters. Also, write the altitude of your analysis.

### **Author**

Line 4: deleted

Line 6: Exactly. We added the period in the statement as well as table 1.

Line 8: Yes, the measurement response is the summation of components of the averaging kernel, which gives the impression how much information comes from measurements on the retrieved state. We added the explanation in the text.

Line 14-15: Main reason is the boundary effect especially at the lowest layer. Because SMR does not have good precision below 18km, assimilated tracer fields contains some discrepancies induced by noise of the measurement.

Line 17: We have changed the text.

Line 23-25: We have added the date (day of year). We also referred to Kuttippurath and Nikulin 2012.

Line 26-28: We have cited the articles.

## **Page 7898**

### **Reviewer:**

Lines 1—2: How did you analyse the airmass transport from Pacific? If this is not from your study, you need to cite an appropriate publication.

### **Author**

We derived from the Odin/SMR N<sub>2</sub>O data. Please see the following cite for example.

[http://www.rss.chalmers.se/~jo/SMRquicklook/Qsmr-2-1/N<sub>2</sub>O\\_5018/gm/](http://www.rss.chalmers.se/~jo/SMRquicklook/Qsmr-2-1/N2O_5018/gm/)

### **Reviewer:**

Lines 4, 14: State the period of vortex break-up.

Lines 21—23: Vortex break-up by February 20? Then why do you show “vortex averaged ozone loss in March” (Figure 8)?

### **Author**

Even in March we still had the vortex. Wohltmann et al. 2014 reported that the vortex was splitted into two parts on 11 February and these parts remained stable until when they were united again (1 March). The reunited vortex then remained stable for some weeks and finally dissipated. We also mentioned that but our article did not have the statement. We have added more explanation about it in Sec. 4.1 and cited Wohltmann et al. 2014.

## **Page 7899**

### **Reviewer**

Line 2: “more features and larger variation” of what?

Line 7: “Another important”

Line 9: Greater than 65 S? So what about 65 N (which is more relevant to this study)?

Line 10: This is an ideal case for Arctic vortex, which is seldom stable and isolated.

Lines 20—21: Yes, but after the SSW there are significant differences.

**Author**

Line 2: Of ozone distribution. SMR ozone has fewer measurements and more noise. Those make an assimilated ozone map more complex.

Line 7: fixed

Line 9: Sorry for the mistake again. 65N should be correct.

Line 10: We agree. Normally the vortex is not so stable in the Arctic. However in the end of December 2009 the vortex was stable enough and well isolated.

Line 20-21: Of course there are differences between the results from the two instruments. As in lines 2—4, the number and quality of measurements create the discrepancies. We described the differences and the reasons in the text.

**Page 7900**

**Reviewer**

Lines 1—2: Please specify the “significant” loss/value. The term significant is relative and hence, write the magnitude of that loss, with altitude and period.

Line 3: State the scale of the depletion occurred during the period, along with the loss rates.

**Author**

Line 1-2,3: We have added the value.

**Reviewer**

Line 4: I do not see the equilibrium in your analyses.

**Author**

We agree that this statement is probably an exaggeration. We have changed it.

**Reviewer:**

Lines 5—6: Please state the mechanism for this ozone loss, as there are no PSCs and chlorine activation at these altitudes during the period. For instance:

Kuttippurath et al. (2010) have given a detailed analysis of this winter in comparison to other winters in terms of ozone loss chemistry. Discuss their results here.

**Author**

The loss at around 650K was discussed in last paragraph of section 4.2.

**Reviewer:**

Please explain the reasons for the ozone production in the lower stratospheric altitudes (Figure 8). Also, please check your data and modelled tracer again.

**Author**

Figure 8a and b show the difference between the assimilated ozone and passive ozone. The minor ozone increase smaller than 0.1 ppmv was created by the noise of the measurements. On the other hand, an ozone increase of 0.2 ppmv occurred from 15 Jan. to 20 Feb. above 750K. The maximum increase is approximately 0.4 ppmv on 14 Feb. at 800K. This increase can be explained by the horizontal mixing. In this period the stratospheric dynamics was complex and the vortex was weak due to the SSW. O<sub>3</sub>-rich air from lower latitudes reached the vortex edge. If the air inside/outside the vortex edge mixes, we may see such an enhancement.

**Reviewer:**

Was there any solid vortex in March, after the major warming in early February, to make a meaningful/reasonable ozone loss analysis? You state later in this section that the vortex broke-up by 20 February too. Furthermore, Dornbrack et al. (2012) show that the final warming was by around that date.

Lines 7—8: I suspect there is hardly any solid vortex in March after the warming in February. It is better to show the average profile in February or both. Can you please explain the large/larger ozone loss in the lower stratosphere in March? In fact, I would expect larger ozone loss around 500 K in warmer Arctic winters.

**Author**

Even in March we still had the vortex. Please check the response for your comments for the page 7898.

Thank you for the suggestion. We have worked that in the revised manuscript.

As we explained in the additional supplement for the question about the vortex edge from anonymous referee #1, our loss estimation in March probably contains more information near the edges where the passive ozone has high value because of the transportation.

**Reviewer:**

Line 8: I do not see these different phases of ozone loss in Figure 8.

**Author**

The first loss was below 500K from 20 DOY. The second loss starts roughly from 60 DOY above 600K.

**Reviewer:**

Line 12: Not sampling issues, but sampling differences.

**Author**

Corrected.

**Reviewer:**

Lines 25—28: I would expect the maximum/activated ClO values of around 1.5 ppbv, but those are about 3 times lower here? In addition, “enhancement of ....”, how much is this enhancement?

**Author**

There are two possible reasons. One is that we only show nighttime ClO thus most of the ClO<sub>x</sub> is in the form of Cl<sub>2</sub>O<sub>2</sub>. The other is the limited vertical resolution of ClO measurements.

We have added an explanation of an enhancement of nighttime ClO. Please also check the response to a related comment from anonymous referee #1.

**Page 7901**

**Reviewer:**

Lines 1—2: Kuttippurath et al. (2010), has already analysed the chlorine activation during this winter with Aura MLS measurements and model simulations. Please discuss and compare your results with their findings.

**Author**

Thank you for the suggestion. We have modified the text involving Kurrippurath et al. (2010)

**Reviewer:**

Lines 3—4: This can be due to the NO<sub>x</sub> chemistry. Kuttippurath et al. (2010) have done a detailed analysis of the contribution of various chemical cycles to the ozone loss in this winter as compared to other Arctic winters. Please discuss their analysis here.

**Author**

Thank you for the suggestion. We have modified the text involving Kurrippurath et al. (2010).

**Reviewer:**

Lines 6—7: Not in all other winters, but mostly in warm Arctic winters (e.g. Kuttippurath et al., 2010).

**Author**

We have added “warm” after “other”.

**Reviewer:**

Lines 8—10: The sentence is not correct (e.g. What are lower altitudes, lower stratospheric altitudes?). References for colder winters such as 2004/2005 are not appropriate here. You could cite the studies for the warm Arctic winters (i.e. winters with major SSW) such as 2002/2003 and 2005/2006 for this.

**Author**

Not altitude but latitude. And the reference cited here is about the NO<sub>x</sub> induced ozone loss in the 2002/2003 winter.

**Reviewer:**

Lines 14—16: Remove this sentence, no need to repeat it.

**Author**

Removed

**Reviewer:**

Line 19: to study polar ozone loss (specify the latitude band otherwise).  
Compare ozone loss values published in other studies with your results. Make a new Section 4.3 for this. Some studies are listed below and search for the missing ones, if any.

**Author**

Thank you for the suggestion and related articles. Now we have made new section for the comparison with other studies of ozone loss in 2010 winter.

**Page 7902**

**Reviewer:**

Lines 2—3: State also the ozone loss values here.  
Line 7: You need to cite a publication which is relevant to this winter as I mentioned before (Kuttippurath et al. 2010).  
Line 13: Merge this paragraph with the previous one by inserting the ozone loss values at appropriate places.

**Author**

Line 2-3: corrected.  
Line 7: We cited Kuttippurath et al 2010.  
Line 13: modified.

**Reviewer**

Table 1: What is this measurement response (averaging kernel/FWHM), please write that in the figure caption.  
Figure 2: Remove “An example” 70 degree, north? If yes, state that. Also write what you mean by measurement response. Instead of N, write “Number of observations” in the Y-axis title, if it is the case. Instead of GE put greater than or equal to sign in the title. Write “potential temperature” for PT.



Figures 3, 5: 70 N (please write the sign of the vortex edge)

Figure 4: Write “potential temperature” for PT; write the latitude sign for EQL (i.e. N or S)

Figure 9: What is active ozone? I thought you were presenting the observations. If not, what are SMILES and SMR here?

**Author**

We have modified the figures in the revised manuscript.

Figure 9: What we presented are not observations themselves. We presented ozone derived by assimilating SMILES and SMR ozone measurements.

**Technical corrections**

**Reviewer:**

Page 7890 Line 1: Profiles are retrieved, not observed by these instruments.

Line 16: initialised “on” Line 19: “ppmv”

Page 7891 Line 3: not irregular, but infrequent Line 9: write something like “showed lowers values ..” than decreased

Page 7892 Line 7: “a few” Line 8: write “Another”, for “An other” Line 14: “other studies” Line 19: delete “also” and state the reason for using the model (e.g. to simulate passive tracer)

Page 7893 Line 20: further “details”

Page 7894 Line 11: described by Frisk et al. Line 19: lines at 501.5 GHz and Line 24: in detail by Jones et al.

Page 7895 Line 9: Delete “model” after the “Data)”

Page 7897 Line 24: not divided by, but “split into” Line 26: not reconnected, but merged (or combined) Line 28: “The period shows”

Page 7898 Line 12: “then the predicted and assimilated results should show the same values”?

**Author:**

All suggested corrections have been done in the revised paper.

# Response for the Review (acpd-14-c1626-2014)

Kazutoshi Sagi

Dear Reviewer,

Thank you very much for your comments on our paper. We will introduce all suggested corrections. Below we present responses to your specific comments and questions.

## Specific comments

### Reviewer:

As the instrument is only operated in 2009/2010, please indicate the fact in the abstract.

### Author:

>> We added the period.

### Reviewer:

It is a bit confusing in the figures not to use one one x-axis. It might help to indicate the day of the year in the global maps and in the text as well.

### Author:

>> The days of the year is also indicated in the brackets following the date for the global maps of N<sub>2</sub>O and O<sub>3</sub>. We fixed that in the text as well

### P7890:

### Reviewer:

3: What does high sensitivity mean here and later on in the paper?

### Author:

>> High sensitivity means that SMILES has low noise on measurements and thus high precision and good measurement response in the height range used in the paper. (see Kasai et al, 2013)

**Reviewer:**

13: How good do SMILES and SMR fields agree?

**Author:**

>> SMILES and SMR fields capture similar features on the maps. On the other hand, SMR fields show larger amplitudes of the variation of ozone and small features that do not appeared in the SMILES fields. We believe that this difference is produced by the noise on the SMR measurements.

**P7891:**

**Reviewer:**

24: the instrument only detects latitude on the south side?

**Author:**

>> This is my mistake. The correct range of altitudes is from 38S to 65N.

**P7892:**

**Reviewer:**

3: How well does EQL70 represent the vortex edge?

**Author:**

>> Please see the response to the related comments from anonymous referee #1.

**P7892:**

**Reviewer:**

3: What is the step width of the measurements?

**Author:**

>> We made a table for the specifications of both instruments.

**P7894:**

**Reviewer:**

11: described by (Frisk et al.,2003), -> Frisk et al. (2013) / also line 24

**Author:**

>> Corrected.

**P7896:**

**Reviewer:**

2: adiabatic vortex descent -> diabatic...

**Author:**

>> We have corrected the wording.

**P7898:**

**Reviewer:**

3: 550K is not in the figures.

**Author:**

>> We have changed the value to match with the figure (above 550K -> at 600K).

**P7899:**

**Reviewer:**

9: 65S?

**Author:**

>> Sorry no. It should be 65N.

**7900:**

**Reviewer:**

Description of Fig.8: Where does the loss (day 12-29, 650K) in SMILES come from? It is not seen in SMR data. And the difference in maximum loss height (day 75-90) between both instruments?

**Author:**

>> Concluding the first point, because the SMILES results reflect not the pole center but lower latitudes near the vortex edge, the apparent loss (day 12-29, 650K) in SMILES is due to an overweighting of the losses near the vortex edge. SMR loss is lower than SMILES below 500K. This is because SMR ozone measurements tend to over estimate ozone at these altitudes due to lower sensitivity/measurement response. We have modified the text.

**Reviewer:**

How do these results compare to findings from other instruments.

**Author:**

>> We will add a new section comparing with other studies.

**Figures:****Reviewer:**

Fig.2a: Globe is too small, maybe also reduce the latitude range. Fig 3. and 6.: Extend the range of date until day 90, similar to the other figures. Increase the size of the figures and the legend. Fig 4./5.: Why not spend some colours to the figures to make them easier readable? Fig. 8: Font size of axes and legend is too small.

**Author:**

>> We will fix all figures to be nice.

# Response for the Review (acpd-14-c1674-2014)

Kazutoshi Sagi

Dear Reviewer,

Thank you very much for your comments on our paper. We will introduce all suggested corrections. Below we present the responses to your specific comments and questions.

## Major issues

### Reviewer:

7890/6: The concept of “ozone loss due to the instability of the vortex” is not very clear. There could be chemical ozone loss by various processes but the instability of the vortex itself does not generate ozone loss. It may cause mixing between air masses with different ozone amounts or simply transport of air from mid-latitudes into the vortex.

### Author:

>> This is badly formulated. We have removed the text, “due to the ~~~” from the revised manuscript. The original intension was to attempt to explain that due to its instability the vortex became accessible to SMILES, despite the limited latitude coverage.

### Reviewer:

7896: The choice of  $\omega$  as a symbol for vertical velocity for different vertical coordinates is confusing, as it is typically used as the vertical velocity in pressure coordinates ( $\omega = dp/dt$ ). It is further well known that slow vertical motions in the stratosphere as the tropical ascent in the Brewer Dobson Circulation but also the descent in the polar vortex can be described best using the heating rate and potential temperature as vertical coordinate. Therefore it would be better to leave out this discussion and refer to the literature.

**Author:**

>> Indeed. We changed the symbol to  $w$ . On the other hand, we need to keep the explanation since it describes the modifications to the model compared to earlier versions.

**Reviewer:**

7896, formula 3: This can only be a necessary condition and is not a sufficient condition. E.g. by increasing the  $\Delta\Theta$  to a very large number you could fulfil this formula, but you may not be able to simulate vertical descent.

**Author:**

>> We agree. The text has been modified in the revised paper.

**Reviewer:**

7897/17: The US-standard atmosphere profile does significantly differ from a polar ozone profile that should rather be used here

**Author:**

>> This is wrong explanation. We have changed the sentence.

**Reviewer:**

7900/4ff: I don't see how you can derive statements about equilibrium between processes from the shown quantity that is an integral of ozone loss rate since the beginning of the winter. Also, the photochemical production in polar spring is probably very low. With the shown method, you cannot discriminate changes between chemical ozone loss or transport of/mixing with air-masses from lower latitudes that did experience earlier Ozone loss (e.g. due to NO<sub>x</sub>)

**Author:**

>> We agree that this statement is probably an exaggeration. We have changed it. However, we believe that our method can roughly separate the changes between chemical loss and transport. Indeed it is difficult to bring out the detailed mechanism behind the loss by assimilation alone. Hence we showed other information such as temperature or ClO.

**Reviewer:**

7900/15ff: The process of polar ozone depletion by chlorine activation and subsequent polar ozone depletion is generally known and must not repeated here. Especially it is not necessary to provide detailed information on PSC types or denitrification.

>> We removed the text.

**Reviewer:**

Chlorine activation can be triggered by different PSC types, most important are the liquid particles. The only shown data for that are the SMR ClO data. From the SMR ClO nighttime data, the chlorine activation is best visible if temperatures rise and thermal decomposition of the night reservoir Cl<sub>2</sub>O<sub>2</sub> becomes important. This cannot be verified as the chemistry is not simulated by the model. However, there are several papers that describe ozone depletion, denitrification of the winter 2009/2010.

**Author:**

>> We agree. Since the partitioning of ClO/Cl<sub>2</sub>O<sub>2</sub> is temperature dependent, the enhancement of nighttime ClO in the end of Jan. has to be the result of thermal decomposition of Cl<sub>2</sub>O<sub>2</sub>. The peak of ClO at 475K on 28-29 Jan. corresponds to the rise of temperature after SSW. On the other hand, the nighttime ClO increased from 16 Dec. (-15DOY) below 500K with 0.1 ppbv. The average of ClO during the period from 16 Jan. (15DOY) to 15 Feb. (-45DOY) is approximately 0.25 ppbv. This increase of ClO during night should be caused by the chlorine activation on PSC. We have modified the statements in the revised paper.

**Reviewer:**

Do you use equivalent latitude >70°N as definition vortex edge throughout the paper? Is this justified for all times and altitudes? Please give an indication of the breakdown time of the polar vortex in the different altitudes.

**Author:**



>> Yes, we used equivalent latitude of 70N as the vortex edge for all times and altitudes. In the revised paper we have also presented ozone loss derived with a potential vorticity criteria (38PVU) in the conclusion chapter.

We have attached the additional pages at the end of this response for more details. Please see the pages for the vortex edge criteria.

**Reviewer:**

Classify the results with respect to other published ozone loss estimates.

**Author:**

>> Another reviewer also suggested including a comparison with other studies. We will add a new section for the comparison at the end of the discussion part.

**Minor Issues**

**Reviewer:**

7890/11: Mention which data from ECMWF are used (operational analyses, re-analyses...)

**Author:**

>> We used the operational analyses of ECMWF. We have mentioned that in the revised paper.

**Reviewer:**

7890/13: "cross-isentropic tracer transport": Do you mean vertical tracer transport or isentropic transport across the vortex edge?

**Author:**

>> We mean the vertical component of the cross-isentropic transport.

**Reviewer:**

7890/25: rather write "... release of active chlorine species (Cl, ClO)"

**Author:**

>> We changed it.

**Reviewer:**

7891/1: The Arctic vortex is also stable. Write rather "less stable"

**Author:**

>> We changed it.

**Reviewer:**

7891/5: As indicated also later, this is only true for the beginning of the winter (Dörnbrack et al., 2012)

**Author:**

>> We agree. We changed the text.

**Reviewer:**

7891/24: This latitude range cannot be true. It should be also in the Northern hemisphere

**Author:**

>> We agree and changed to the correct value.

**Reviewer:**

7892/5: rather write "SMILES does not measure inside the vortex..."

**Author:**

>> We changed the text.

**Reviewer:**

7892/17: remove "us"

**Author:**

>> done

**Reviewer:**

7893/6: please re-phrase, as it could be mis-understood. The limb emission is neither coming from the ISS nor from the 340-360km range.

**Author:**

>> We changed this.

**Reviewer:**

7895/25ff: This is a strange concept. The diabatic descent of the air masses in the polar night is caused by the radiation imbalance (no solar irradiance). This must be re-phrased.

**Author:**

>> We think the statement is correct but it might lead the misunderstanding. We have changed the text in the revised article.

**Reviewer:**

7897/26: rather write "...vortices did reconnect by..."

**Author:**

>> done

**Reviewer:**

7897/28: rather write "lowest temperatures"

>> done

**Reviewer:**

7898/11: rather write "...would be perfectly simulated..."

**Author:**

>> done

**Reviewer:**

7899/5: If there is a known bias, please mention the order of magnitude

**Author:**

>> We have modified the text.

**Reviewer:**

7899/9: Probably you mean 65°N

**Author:**

Yes. Changed.

**Reviewer:**

Figs 3 and 6: The figures should be displayed larger such that the details can be visible

**Author:**

>> Figures have been modified.

**Reviewer:**

Fig. 8: Are the data displayed in Fig. 8 also averages for equivalent latitude >70°N?

**Author:**

>> Yes, we use equivalent latitude of 70°N for all vortex mean calculations.

## Appendix: The vortex edge criteria

Here we explain how the equivalent latitude of  $70^{\circ}\text{N}$  works for the vortex edge in the 2009/2010 Arctic winter.

There are several descriptions for the vortex edge used in previous studies. Here we show two candidates.

1. Equivalent latitude (EQL) criteria :  $70^{\circ}\text{N}$  for this study.
2. Lait's potential vorticity (PV) criteria : 38PVU [ $1\text{PVU}=10^{-6}\text{Km}^2\text{kg}^{-1}\text{s}^{-1}\text{deg}^{-1}$ ] (eg. Hommel et al. 2014)

Figure 1 shows the assimilated  $\text{N}_2\text{O}$  maps computed by DIAMOND using Odin/SMR  $\text{N}_2\text{O}$  measurements on selected dates and different potential temperature levels. The black and white lines indicate the EQL of  $70^{\circ}\text{N}$  and PV of 38PVU, respectively. The PV edge ( $=38\text{PVU}$ ) and EQL edge ( $70^{\circ}\text{N}$ ) match until ~20 February. After that period the EQL edge covers a larger area than the PV edge. In this winter, the vortex was split into two parts after a major SSW in mid-February. Then these parts were merged on 1 March. From the date when the vortex was reunited, the EQL edge differs from the PV edge. The EQL value corresponding to the PV edge is approximately  $80^{\circ}\text{N}$ . Comparing with the EQL edge, the PV edge is more consistent with the area where  $\text{N}_2\text{O}$  has a relatively high gradient. This means that our estimation of loss using the EQL criteria probably contains a larger contribution from the transition area near the vortex edge after 1 March.

Figure 2 shows the vertical profiles of accumulated ozone loss derived from the assimilation of SMILES and SMR ozone measurements as of the end of February (58DOY) and the end of March (89DOY). The solid lines are for using the EQL edge and the dashed lines are for the case of the PV edge. Major differences between the two criteria can be seen above 800K on 28 February and below 550K on 31 March. The PV criteria show roughly 0.2 ppmv higher loss above 800K. The  $\text{N}_2\text{O}$  maps on potential temperature of 800K for 10 days before 28 Feb. (not shown here) show large variations and the standard

deviation of the ozone inside the vortex (not shown here) is large for the period of the vortex separation. Probably the horizontal mixing causes the difference. The other major difference on 31 March below 550K is approximately 0.3 ppmv. It is likely that the air near the vortex edge moderates the loss of ozone using the EQL criteria. However the losses derived with two criteria agree with each other within 10%.

We will replace figure 9 in the manuscript to the loss profiles shown in this supplement and modify the text.

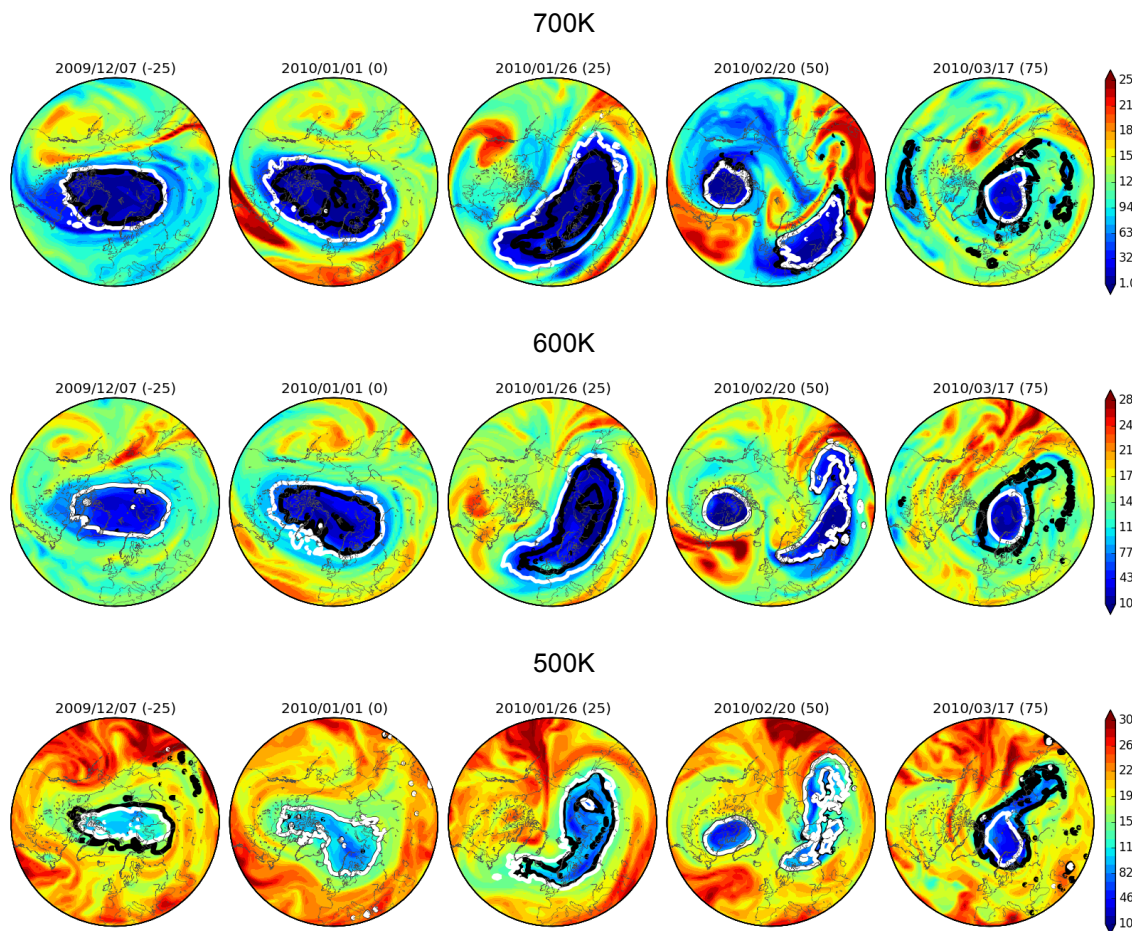


Figure 1. Assimilated N<sub>2</sub>O maps for selected dates and potential temperatures. Black and white solid lines show the equivalent latitude of 70°N and the Lait's potential vorticity of  $3.8 \times 10^{-5} \text{ Km}^2 \text{ kg}^{-1} \text{ s}^{-1} \text{ deg}^{-1}$ , respectively.

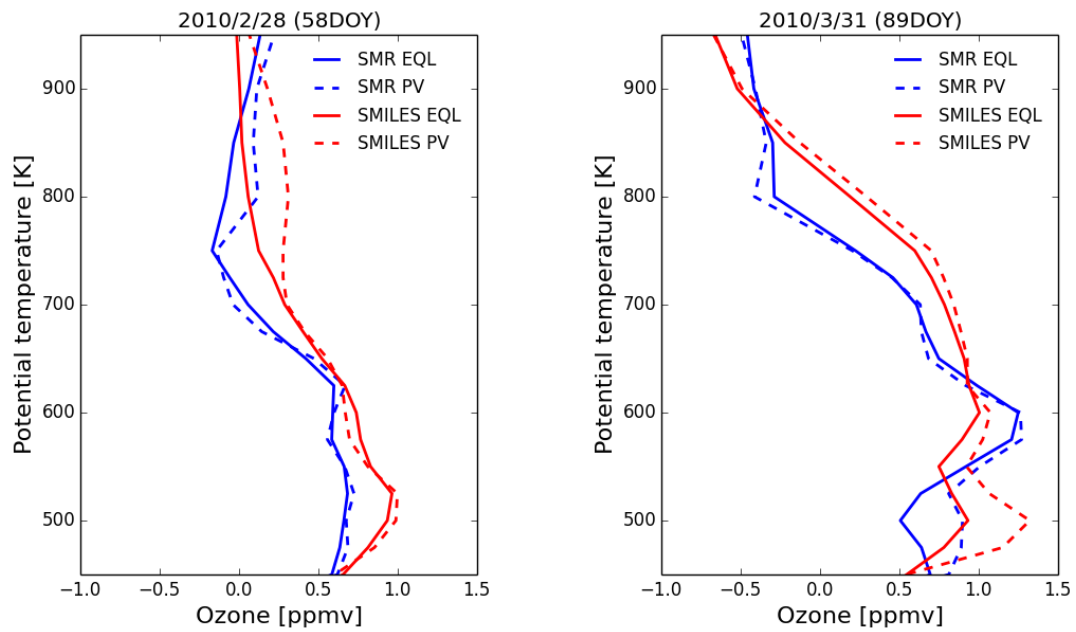


Figure 2. Vertical profiles of accumulated loss on selected dates (58DOY and 89DOY). Red and blue colors indicate the result derived from SMILES and Odin/SMR measurements, respectively. Solid lines show the results obtained with the EQL criterion and dashed lines are for the PV criterion.