

REPLIES TO THE COMMENTS OF REFEREE #1

General comments: The main scientific focus of this manuscript is on comparing chemical ozone losses and related chemical and dynamical processes during the Arctic winters 2010/2011 and 1996/1997. Both winters were characterized by fairly late break-downs of the Arctic polar vortex, comparatively low temperatures in the lower polar stratosphere in February and March. In contrast to the winter 2010/2011 the lower stratospheric temperatures in the Arctic vortex were relatively high in December and January in 1996/1997, explaining the markedly smaller chemical ozone losses during the latter winter. The paper provides an interesting overview of the similarities and differences between the two winters studied. I have no really major objections to the publication of this paper, but ask the authors to consider the specific comments listed below.

Thank you for recommending publication of this manuscript.

Specific comments:

Page 6878, lines 11 – 14: ‘This coincides with ... and a ... and about ... and about ...and about’ I suggest splitting this sentence in two sentences.

Done. Please find the split sentences in [Abstract](#), [Lines 14–19](#)

Page 6879, line 26: suggest to add ‘a description of’ to read ‘is succeeded by a description of the data and ..’

Done. Please find the revised sentence in [Page 2](#), [Paragraph 2](#), [Lines 1–4](#)

Page 6882, line 6: ‘cold temperatures’. I believe temperatures can just be high or low, but not warm or cold. Same comment on Page 6883, line 1.

We have changed the sentences in [Page 3](#), [Paragraph 1](#), [Line 14](#) and [Paragraph 2](#), [Line 28](#)

Fig. 3, second panel from top (CIO): There seems to be a discontinuity in the contour plot at 475 K, particularly visible during the period with enhanced CIO between day 30 and 90. Is this supposed to be like that? What may the origin be?

It depends on the temperature structure of the lower stratosphere. There was slightly higher temperature in that period, which is reflected as a small gap in the PSC areas too, as shown in [Figure 4](#). Therefore, there is no chlorine activation and lower CIO values as compared to the adjacent periods.

Page 6884, last paragraph and Fig. 4: The MLS ozone observations (middle right panel) show a very strong change in the slope of the contour lines around day 30. What may the origin of this effect be? A similar feature is not present in the Mimosa-Chim ozone at all. A simple explanation would be that not only air masses inside the vortex are used for the MLS mean. I’m also wondering whether the abrupt change in slope around day 30 is possible at all if the data sets are smoothed with a 7-day window? Perhaps I’m missing a point here.

Thank you for this. We noticed that our data selection was not perfect there. Therefore, some “bad” profiles were included in this analysis. We have excluded those profiles now. Please find the revised [Figure 4](#). Since there is no significant change in the results, there is no change in the text. Please find the data selection procedure in the revised text in [Page 2](#), [Paragraph 4](#), [Lines 17–29](#)

The smoothing is performed with the available profiles. If there are only 3 profiles between the data gaps, then it is done only with those three profiles.

Fig. 4: It would be good to mention in the Fig. caption what the interval between adjacent contour lines is in case of the bottom panels. I assume the contour line without label is the 0.5 ppmv contour?

Done. Please find the revised [Figure 4](#) caption.

Page 6886, lines 13 – 18: I'm not able to fully understand how exactly the PSC sensitivity studies were done. If NAT PSCs are not considered, then a maximum ozone loss of 1.8 ppm is modeled, but what do you mean with 'and a relative loss of about 65 % for the control run'. Perhaps I'm missing a simple point, but I read this sentence many times and it's unclear to me what exactly was done. Please clarify.

We have revised the paragraph. Please find it in [Page 4, Paragraph 6](#)

Page 6887, line 27: 'However, in February–March 2011, our analyses show exceptional contributions from the ClO-ClO (45 %) and BrO-ClO (35–40 %) cycles in terms of absolute values in the lower stratosphere at 475 K.' Looking at Fig. 6b there are not really large differences in the contributions of the ClO – ClO as well as the BrO – ClO cycle between 1996/1997 and 2010/2011. Therefore the statement appears a little misleading. Or is the statement intended to compare 2010/2011 with other winters (not including 1996/1997) which are not shown in this Figure? Or perhaps the statement actually refers to the absolute losses associated with these reactions? Then, reference to the top panels would perhaps be appropriate. If this is the intended meaning then the statement is not correct for February (looking at the top left panel of Fig. 6a), but only for March.

Although the relative contributions (%) look identical, the contributions in absolute values were larger in 2010/2011 in tune with the higher ozone loss and loss rates. This is what we meant by the sentences. We have revised the text in [Page 5, Paragraph 3, Lines 9–14](#)

Page 6888, line 9: 'The contributions of various chemical cycles during the winter 2010/2011 thus stand in contrast to those in other Arctic winters' Please mention what exactly the differences are.

The duration was longer and the amounts of contributions were also higher in 2010/2011 as compared to those of other Arctic winters. This is stated in [Page 5, Paragraph 3, Lines 25–32](#)

Page 6888, line 11: 'Since these are (presented in Fig. 6b) the fractional contribution of the individual cycles to the cumulative ozone loss that occurred at the respective altitudes, the relative contribution of various chemical cycles in 1996/1997 also shows comparable values to that of other Arctic winters.' The meaning of this sentence is unclear to me. Can you clarify? I'm not sure the logic of this sentence is correct. Moreover, the second part of this sentence appears to contradict the previous sentence (my previous point). I apologize if I'm missing an obvious point here.

As mentioned earlier, although the relative contributions look similar in both winters, their contribution in absolute terms are different and are in agreement with the ozone loss in each winter. We have revised the text in [Page 5, Paragraph 3, Lines 9–13](#) and [Lines 26–37](#)

Page 6889, line 20: Can you offer an explanation for the differences of the results based on total column measurements to all the other (consistent) results mentioned in the previous lines?

This could be due to the differences in ozone loss computation method, data, simulations and vortex criterion used for the loss estimation. In addition, the differences in the vertical column range considered for the ozone loss estimation can also contribute to these differences. These are mentioned in [Page 6, Paragraph 2, Lines 22—25](#)

Page 6889, line 22: 'by the end of each month .. until mid-April' This is somewhat inconsistent and I suggest changing it.

Done. Please find the revised statement in [Page 6, Paragraph 3, Lines 3—5](#)

Page 6890, line 6: Is there a simple explanation why the results published by Balis et al. (2011) differ significantly from all the other studies mentioned?

In addition to the reasons described in [Page 6, Paragraph 2, Lines 22—25](#), there are two other important reasons for these differences. (i) The differences in the passive tracer calculations (please find the short comment) and (ii) the differences in the calculations of vortex area, as they consider a latitudinal segment north of 60N and are not exactly the vortex average. These changes can make drastically different results and hence, all other estimations presented here are not directly comparable to the results of Balis et al. (2011). However, they have a new analysis with similar passive tracer with a vortex criterion at 475K, which yields comparable values. We have mentioned these in [Page 6, Paragraph 3, Lines 19—36](#)

Section 3.5, Comparison with other Arctic winters: I suggest also mentioning the recent study by Sonkaew et al. in ACP(D)? dealing with ozone loss determination in several Arctic and Antarctic winters using SCIAMACHY ozone data.

Cited. Please see that in [Page 6, Paragraph 4, Line 5](#)

Page 6892, line 1: 'We use the same model and Aura MLS measurements' ? What exactly do you mean here?

The model set-up (input, coefficients, etc) is the same for these Antarctic runs. Also, the Aura MLS measurements are used for the comparisons. We have changed the sentences. Please find the revised statement in [Page 7, Paragraph 2, Lines 4—14](#)

Typos etc.:

Page 6879, line 24: remove 'a' in 'with a high resolution . . . simulations'

Page 6889, line 17: 'simulated from REPROBUS' -> 'simulated with REPROBUS' ?

Page 6897, line 8: 'Froide- vaux'

Page 6897, line 10: 'Naka- jima'

All are corrected.

We are grateful to REFEREE #1 for his/her critical review. The comments were really helpful in drafting the revised version of the manuscript. We do appreciate the time he/she spent for the evaluation this manuscript.