

***Interactive comment on* “Chemical ozone loss in Arctic and Antarctic polar winter/spring season derived from SCIAMACHY limb measurements 2002–2009” by T. Sonkaew et al.**

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Interactive comment on  
"Chemical ozone loss in Arctic and Antarctic polar  
winter/spring season derived from SCIAMACHY limb  
measurements 2002–2009" by T. Sonkaew et al.

Reply to Anonymous Referee 2

12 October 2011

### General comments

Reviewer comment: a) Scientific significance: good

Reviewer comment: The manuscript describes inter-annual variability of ozone loss occurring during winter and spring in the polar lower stratosphere of both hemispheres. The analysis is based on a 7-year ozone data set of the space-borne SCIAMACHY instrument on Envisat. This work, together with other related studies based on different instrumental techniques and analysis methods, has the potential to contribute to a better understanding of the inter-annual variability of chemical ozone loss, notably in the meteorologically very variable Arctic lower stratosphere.

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Reviewer comment: b) Scientific quality: fair

The study comprises an in-depth analysis of polar stratospheric ozone loss in both hemispheres as observed by SCIAMACHY. The methodology chosen by the authors, based on calculation of daily vortex average ozone fields, is in general justified. I have nevertheless three major and several specific points which needs to be addressed before the manuscript can be recommended for publication in ACP:

Reviewer comment: The basic limitation of the SCIAMACHY data set is due to the fact that ozone measurements are taken only during day-time, thus not in the polar night region. The vortex fraction sampled by the instrument increases basically from winter solstice to spring, if the vortex is stable and centred around the pole as in the southern hemisphere. Although this basic limitation is well described in the manuscript (Figures 1 and 2), the consequences for the observed ozone loss are unfortunately not properly discussed in the relevant sections, notably when comparisons are made to results obtained from other observation techniques and analysis methods providing real vortex averages. For example, SCIAMACHY samples during August and January only the sun-lit part of the vortex, that is the region where ozone loss occurs. The observed ozone loss rates are however neither representative for the entire vortex nor are they vortex averages, and this needs to be properly addressed throughout the manuscript.

Response: We agree with the reviewer that the SCIAMACHY sampling of the polar vortex is limited, particularly during the early parts of the periods studied. We now mention explicitly what the basic assumptions of the method are and that limited sampling may be an issue. We would like to point out that several of the earlier publications on this subject suffer from the same problem. Ozone loss determinations based on solar occultation observations are also based on near-terminator observations (with typically even less observations in the vortex) , without any nighttime observations

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available. Moreover, we would like to point out that the good agreement of the chemical ozone losses found in this study with other studies (see extended section 5 on comparisons) indicates that the basic assumptions of our methodology are essentially justified.

Reviewer comment: There are several specific issues which require a further iteration of analysis (see specific comments below). For example, ozone loss estimates in the Antarctic vortex 2002 are fairly large already in August, in contrast to other published studies. Also, for the Arctic winter 2004 negative ozone loss values have been obtained. Explanations are basically missing. These issues, among others, should be resolved or properly discussed and explained in the manuscript.

Response: We tried to address these points in our replies to the specific comments below. Regarding the year 2002, we agree that the ozone losses shown in (now) Fig. 11 appear somewhat larger than in the other years. At the 475 K level – as shown in Fig. 9 – this is not that obvious. Following the suggestion by the reviewer, we checked our calculations again, but were unable to find any anomalous behavior for the analysis of the year 2002. The relatively large losses may also be affected by the choice of the reference date. As (now) Fig. 9 shows the 2002 losses during the first week of the considered period are already about 0.5 ppm. If we had taken a later reference date, then the absolute losses would have been smaller. We added a brief discussion on this to the text. Regarding the northern hemisphere season 2003/2004 possible reasons for the negative ozone loss values are now discussed in the manuscript.

Reviewer comment: Comparisons of ozone loss results with earlier work needs to be more comprehensive if this work should be of any value. So far only one winter is discussed, and only a limited number of ozone loss studies are considered. Potentially, a relatively long satellite data as the one presented here could provide a link between ozone loss estimates published earlier for individual winters based on a variety of

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observational techniques. I feel that this part of the manuscript needs a major effort to become more comprehensive.

Response: We agree, the discussions and comparisons in the initial version of the manuscript were quite limited. We tried to improve this by markedly expanding section 5 dealing with comparisons to earlier studies. In section 5.1 comparisons for the Arctic winters are provided, and section 5.2 discusses comparisons for the Antarctic winters.

Reviewer comment: c) Presentation Quality: fair

The paper is in general fairly well written. Specific technical recommendations concerning the manuscript text are given below (technical comments).

However, the figure labels and legends are far too small for most of the figures. I think the paper should have never passed the initial ACPD technical review stage (this is only partly an error of the authors) and recommend re-submission of a considerably revised version with improved figures in publication quality.

Response: Thank your for pointing this out. We agree that the figure labels of some of the figures were quite small. But part of the problem is also that the Figures included in the online pdf version have degraded resolution. If I open individual figures everything is very legible, and not blurred at all. In the revised version of the paper the size of the axis labels, tick labels and Figure panels was increased and we checked that everything is legible if at the 100% zoom level.

### Specific comments

Reviewer comment: Fig 3 and 8: The figures show that fitting a straight line to the vortex average ozone evolution is clearly inappropriate and should be omitted.

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Response: We agree with the reviewer and the linear fits and the discussion of the subsequent results have been removed.

Reviewer comment: Table 1 and 2: Consequently, I suggest to remove the linear fit results from the tables and figures, as ozone loss rates are not constant with time over the entire winter/spring period. Suggest to provide rather the accumulated ozone loss (in ppmv) in the tables, for carefully selected levels (keeping the 4.5km altitude resolution in mind). The variability of the ozone loss rates could instead be shown in the figures, if the data set provides this information with reasonable quality. Tables for NH and SH can be merged and the manuscript text (e.g. p6566, p6570) should be modified accordingly.

Response: Thank you for this point, we added information on the maximum accumulated ozone loss and the isentropic level at which this maximum occurred. Note that the tables for the northern and southern hemisphere are still separated because the considered periods vary from year to year in the NH.

Reviewer comment: Fig 5: The negative values of ozone loss for the 2004 winter might indicate limitations of the employed method. Uncertainties need to be discussed in the text.

Response: We added more discussion on this in the revised version of the manuscript.

Reviewer comment: Fig 7: According to this figure, the largest ozone loss in the Antarctic is observed in 2002 (already 1ppm in August, 2.5ppm by mid-September). In particular, in 2002 and 2007 ozone loss seems to occur earlier than in the other years. This results is unexpected, if compared to published literature on the subject,

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and needs to be justified, if true. The authors are advised to double-check their calculations. So far, I couldn't find a discussion of this in the manuscript.

Response: We checked the calculation again, and were not able to find any errors at all.

Reviewer comment: Fig 12: Very nice agreement with other studies, but only for a selection of data sets. I wonder how this would look if a more comprehensive analysis would be made, taken all published results for this winter into account. How consistent are the SCIAMACHY and other published ozone loss estimates for the different winters under consideration?

Response: We attempted to make a quantitative comparison by selecting all publications which have calculated the chemical ozone loss for the same period, i.e. from 5 Jan to 25 Mar. The comparison could certainly be extended, but given the good agreement with all of these other techniques – which indirectly verifies our methodology (at least for the winter 2004/2005) – we believe it's not necessary to include even more studies. But – as mentioned above – section 5 on comparisons of our ozone loss estimates with independent studies has been extended.

Reviewer comment: Introduction l15-22: The classification of techniques (and list of references) provided here is certainly incomplete and needs to be improved. Also, the study of Singleton et al. (published in 2007) discusses only a selection of techniques. There are for example newer techniques, e.g. using data assimilation techniques, and different methods when it comes to correction for diabatic descend. It is suggested to widen the scope of the introduction by taking more recent publications on the subject into account. As there are various slightly different methods calculating vortex

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averaged ozone loss it is suggested to (at least) replace "the vortex average method" by "a vortex average method" throughout the manuscript incl. the abstract. Moreover, multiple use of "vortex average method" in the manuscript is particularly irritating as SCIAMACHY actually doesn't provide vortex averages, but samples only the sun-lit part of the polar vortex. This can certainly be improved by using more appropriate wording throughout the manuscript.

Response: The reviewer is right that the fact that we only use measurements in the sunlit part of the vortex for the analysis needs to be made clearer. However, we note that many of the earlier studies using a vortex average method essentially suffer from the same "problem". The POAM III analysis by Hoppel (2002) is based on solar occultation observations, which sample the edge of the sunlit part (and have a worse sampling than the limb scatter observations used here). The same is true for the POAM II study by Bevilaqua (1997). The study by Eichmann et al. (2002) is based on GOME ozone profile retrievals in the sunlit part of the vortex. We adjusted the text emphasizing that only the sunlit part of the vortex is observed by SCIAMACHY.

Furthermore, we extended the discussion of other techniques used to determine chemical ozone losses in the polar vortices. And, following the reviewer's suggestion, we only use the phrase "a vortex average method" rather than "the vortex average method" throughout the whole manuscript.

Reviewer comment: Section 2 Can you indicate typical local times of the SCIAMACHY observations in the polar regions during the relevant periods?

Response: For latitudes equatorward of about 60 deg the local time is within 1 hour of 10:00 a.m. At polar latitudes SCIAMACHY 'flies' quickly through local time with the local time at the northernmost point (82N) being 4 p.m. and at the southernmost

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point being 4 a.m. At 80 N / S the local time of the measurements is close to 1 p.m. / 7 a.m. This is now briefly discussed in section 2.

Reviewer comment: How good agree SCIAMACHY ozone measurements in the polar regions with other observational data sets, are there any known errors or biases?

Response: The agreement of the SCIAMACHY ozone concentrations within independent satellite measurements is typically within 10% in the stratosphere, including the polar stratosphere. We added a brief discussion on this to section 2.1.

Reviewer comment: Section 3 p6563, l18-19 "evident": please explain why SCIAMACHY sampling is sufficient for studying ozone loss in the polar vortices using a vortex average method. This is not evident at all: SCIAMACHY doesn't sample the entire polar vortex during all times, hence vortex averages cannot be calculated. According to literature, ozone loss typically starts in January (NH) or August (SH) in the sun-lit, outer parts of the polar vortex. Please discuss sampling limitations of SCIAMACHY observations more carefully. The information is already nicely provided in Fig. 1 and Fig. 2, but the authors do not draw the obvious conclusions.

Response: Limitations of SCIAMACHY measurement and the used method were added.

Reviewer comment: Section 4 It is not so relevant to describe the different employed software packages ("SODD", "FUDD").

Response: It's certainly not important from a geophysical point of view, but we believe it

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is relevant to mention what software was used to determine the chemical ozone losses and decided to leave the software names in the manuscript.

Reviewer comment: I would rather suggest to focus on a step-by-step description of the adopted method for calculating chemical ozone loss. E.g.: How is the vortex defined?

Response: This is a very good suggestion and we tried to incorporate this in section 4. Note, however, that the our vortex criterion is still explained in section 3.

Reviewer comment: Are ozone profiles interpolated on potential temperature levels using UKMO data?

Response: Yes, this is exactly what is done and this point has already been mentioned in the last line of section 2.1.

Reviewer comment: How is the diabatic correction from equation (1) applied, to individual ozone profiles or vortex averages? What is the intra-vortex variability of this correction? Some information on the method for the diabatic transport correction is already given, but other information is provided elsewhere. Suggest to regroup relevant information and rewrite this section.

Response: Yes, the correction is applied to vortex averages. The methodology has been rewritten and is now explained step by step in Section 4.

Reviewer comment: Section 4.1 p6565, l9-15: The choice of the reference date for

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the ozone loss calculation is indeed arbitrary. For comparisons with other studies it would be better to choose a more common reference, for example the average ozone during the first 10 days or two weeks of January (NH) and August (SH). Explain why the choice of the reference should only weakly affect the derived relative ozone losses, this is not evident.

Response: We agree with the reviewer that for comparisons with other studies it would be better to have the same reference date for every season. However, due to interannual variability of the vortex evolution SCIAMACHY starts sampling the vortex at different times, which is one of the main reasons for the different reference dates shown in Table 1 (ranging from Jan 7 to Jan 26). In order to use a common reference and still be able to include all seasons in the analysis we would have to use the latest of these reference dates, implying that we're throwing away almost 3 weeks of data in the worst case. Therefore, we believe it to be a better compromise to use varying start dates.

Perhaps the statement that the choice of reference dates affects the derived ozone losses only weakly is optimistic and quite vague if "weakly" is not defined further. What we wanted to say, is that the main conclusions of the study (significant ozone losses during the cold winters and essentially no ozone losses during the warm winters) are not affected by the choice of reference date. We adjusted the text and removed "weakly" to reflect that.

Reviewer comment: I21-23: Can mixing (within the vortex or with air from outside) play a role here? The Arctic vortex split in early March 2005, associated to a warming event.

Response: We have mentioned this impact in p.6568, lines 22-26.

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Reviewer comment: I24: Major warming events occurred also in 2006 and 2009. Why do you get negative values only in 2004?

Response: We don't really have a full answer to this question. We believe the variability in the derived ozone losses in 2004 are due to a combination of sampling and possible exchange across the vortex boundary not considered in this study. In 2006 and 2009 the time series are also significantly shorter than in 2004. Perhaps the same issue would have arisen there if the time series were longer. We added some discussion to the manuscript.

Reviewer comment: p6566, I1-3: What exactly is subtracted (daily averages, individual profiles) to calculate chemical ozone loss? This could already be explained in Section 4 (see relevant comment above).

Response: This is now also addressed in the new description of the methodology. Please see above.

Reviewer comment: p6568, I1-9 and Fig7: SCIAMACHY sampling characteristics need to be included in the discussion of the observed variability.

Response: Regarding to L1-9, p6568, the data gap in 2006 between the 500 K and 550 K isentropic levels was because no potential vorticity  $>38$  MPVU that can be detected for polar vortex criterion in SCIAMACHY observation's view. The sampling problem might just be a minor cause for the gap but SSW would be the major cause. Kattipurath, 2010, also did not consider data and reported that no PSC area beyond

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the late of January for 2006. This point has been described in manuscript and SCIAMACHY sampling characteristic is not necessary described in this section.

Reviewer comment: I14-28 SCIAMACHY ozone loss rates shown in Fig 8 do not reflect vortex averages. The vortex average ozone loss rate would be considerably smaller in August than in September when more light is available for ozone destruction. Moreover, the vortex average ozone loss can not be expected to be linear and stated values for average or fitted ozone loss rates are therefore not very helpful. The discussion of ozone loss rates should be changed accordingly throughout the manuscript.

Response: We now mention several times in the manuscript, that SCIAMACHY only samples the sunlit part of the vortex. The linear fits to the ozone loss time series were removed, following the reviewer's suggestion.

Reviewer comment: The comparison with Hoppel et al. is odd if discussion of sampling characteristics is omitted. Does SCIAMACHY vortex sampling differ between the years, which might explain some of the observed inter-annual differences?

Response: The reviewer is right, that strictly speaking one would have to take the different samplings of the instruments into account. However, since this is only a relatively minor point, we decided to leave it as is, and focus on the bigger issues. The SCIAMACHY geographical sampling is the same every year, which may lead to inter-annual differences in vortex sampling if the vortex evolution differs from year to year.

Reviewer comment: I28 OCIO is not the best indicator for chlorine activation.

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Response: Acknowledged. Other halogen compounds were added together with the corresponding citations.

Reviewer comment: p6570, I6: SCIAMACHY measurements of PSC's in 2002 are apparently inconsistent with the large chemical ozone loss for this year. Can this be explained?

Response: The chemical ozone loss rate was calculated for "inside" polar vortex but PSC itself was low for SH 2002.

Reviewer comment: I18: Attribution to the low altitude resolution of SCIAMACHY: This is certainly one effect to be taken into account, leading potentially to smaller ozone loss, but other effects (e.g. spatio-temporal sampling) need also to be discussed.

Response: The limited sampling of the vortex by SCIAMACHY limb measurements during January is now explicitly mentioned in several parts of the paper.

Reviewer comment: Section 5 p6571: Check inconsistencies between Fig 10 and 11.

Response: We're not entirely sure what inconsistencies the reviewer is referring to. Fig. 10 shows the accumulated ozone loss inside the polar vortex and Fig. 11 shows the ozone mass loss estimation integrated over the polar vortex volume between the 450 K to 600 K isentropic levels (as described in detail in the text), i.e., very different quantities are shown in Fig. 10 and 11.

Reviewer comment: p6571-6572: A comparison with other studies should be more

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comprehensive. See general remarks. E.g. for the NH 2005 winter there are published results from Manney et al., Jin et al., Singleton et al., Rosevall et al., Rex et al., etc.. I think a comprehensive literature research and comparative discussion of SCIAMACHY results for this and other winters would certainly help to improve this paper.

Response: The reviewer is absolutely right. Following the suggestion section 5 was extended significantly and now includes references to numerous earlier studies on chemical ozone losses in both polar vortices.

Reviewer comment: Section 6 and Fig 13: Zonally averaged PSC occurrence rate at 40-65N? Again, sampling needs to be taken into account. Ideally, vortex average PSC occurrence should be related to vortex average ozone loss. The authors should further explain why they think their result (obtained for mid-latitude PSC occurrence) is relevant.

Response: We tried to better explain the meaning of the results and added some text to section 6. Because PSCs mainly occur poleward of about 60° in the northern hemisphere we changed the latitude bin to 50° – 65° N in (now) Fig. 15. We also added the lat/lon grids in the revised version of the manuscript.

Reviewer comment: Conclusions p6574, l24-26: Obvious contradiction - the ozone loss in the Antarctic vortex was not similar in each year. The conclusions are inconsistent with the Figures. Given the large number of specific comments listed here, it is recommended that this section is rewritten once the manuscript has been revised.

Response: We sorry, but we don't understand this comment. Looking at (panels d and e in particular) it is obvious that the absolute (in ppmv) and relative chemical ozone

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losses in the Antarctic polar vortex are indeed very similar for the different years. Perhaps we should have written 'for different years' rather than 'in each year'. This has now been changed and we hope the statement is less confusing.

### Technical corrections

Reviewer comment: Figures 1,2,3, 5, 8, 10: Labels unreadable!

Response: Thanks, we increased the label size.

Reviewer comment: Figures 3 + 6: date.month should be "date"

Response: corrected, thanks.

Reviewer comment: Fig 7, caption: There are no "periods" indicated in Fig 6. Check English in captions. Are data "daily" averages? Any smoothing applied (splines)?

Response: we apologize, 'period times' is certainly not well phrased. However, as the right panel of Fig. 6 indicates, the ozone mass loss determinations for the Arctic winters did not start on the same day (unlike the treatment of the Antarctic winters). The periods considered for the northern hemisphere are limited by the start and end dates of the curves shown in Fig. 6b. We also realize that the x-axis label was not correct (Daily mean of vortex volume), and this has now been changed. The time series shown in Fig. 6b was derived from the daily averaged ozone losses smoothed with a 3-day boxcar function, as explained on p. 6565).

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Reviewer comment: Fig 4 and 9. What is shown here? Which level? What are the units? Colorbar is missing. Latitudes and longitudes should be indicated.

Response: These plots show maps of PSC occurrence rate determined as the ratio of the number of observations with PSC detections and the total number of SCIAMACHY limb measurements in a given latitude/longitude bin. This has been described in detail in section 2.2. Occurrence rate is dimensionless and varies from 0 to 1. PSC altitude is not shown here, but is also estimated from the limb measurements. The new version of the manuscript includes the colorbar as well as latitude and longitude labels.

Reviewer comment: Fig 5 and 10: Reference dates for the ozone loss shown? Indicate in caption!

Response: For the Antarctic winters the reference date is now explicitly mentioned in the Figure caption. For the Arctic winters we refer to the dates listed in Table 1, because they differ slightly from year to year.

Reviewer comment: Fig 3, 6, 8, 11: Uncertainties of the measurements (error bars)?

Response: We added uncertainties to these Figures.

Reviewer comment: Fig 14: Caption accumulated vortex average chemical ozone loss? For which period? PSC detections at 40-65N? I doubt there were PSC's at mid-latitudes, is this a typo?

Response: The periods are now mentioned in the Fig. caption. 40-65N was the latitude

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range used to determine the PSC occurrence rate, but this does not mean that we detected PSCs at 40N. We changed the latitude range to 50-65N in the revised version.

Reviewer comment: p6557 Abstract The abstract can be improved by providing relevant quantitative information on the inter-annual variability of observed ozone loss: e.g. I11: (on Arctic) More quantitative: how much ozone loss was observed in the years with considerable ozone loss, and over which periods of time? How do these periods vary from year to year? How much ozone loss was observed in the other years, for comparison?

Response: More information was added to the abstract.

Reviewer comment: I17: (on Antarctic) "do not vary much" (how much?)

Response: We now quote the range of numbers for the ozone mass loss as well as the vortex volume between the 450K and 600K isentropic levels in the abstract, following the reviewer's suggestion.

Reviewer comment: I19: what is an anomalous year? Reformulate!

Response: We now mention explicitly that a major warming occurred in September 2002.

Reviewer comment: I22-24: see specific comments on relevant section!

Response: OK!

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Reviewer comment: p6558, I4 dominates

Response: Corrected, thanks.

Reviewer comment: p6559, I8 satellite I16 remove "the"

Response: Done.

Reviewer comment: p6560, I7 mid-mesosphere (?) I14 "Stratozone" (?)

Response: L7: we changed text to "lower mesosphere". The data product is called "stratozone" and we now mention this explicitly in the first sentence of section 2.1.

Reviewer comment: p6561

I3 latitude - longitude bin of ...x... ? (provide resolution, as used in this study)

I25 SCIAMACHY samples the vortex also in warm winters (reformulate)

Response: The size of the lat-lon bins used (5x5) was added and the statement in line 25 was reformulated.

Reviewer comment: p6562, I16 Winter 2008-09? (omitted)

Response: Thanks, winter 2008/2009 is now listed as well.

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I19 "..., and from the National..."

Response: Corrected.

I23 What about the major stratospheric warming in winter 2003-04? (omitted)

Response: Thank you, corrected.

Reviewer comment: p6563, I21 suggest to remove "Using the vortex average method,"

Response: Changed.

Reviewer comment: p6564, I23, eq (2): Explain Q and O3d.

Response: Done.

Reviewer comment: p6565, I6 Spline interpolation? Should be shown in the figure along with original data.

Response: More details on the spline interpolation was added to the text. Interpolated values are shown as open circles in Fig. 3a together with the original data (solid circles).

Reviewer comment: p6566, see specific comments

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Response: OK.

Reviewer comment: p6567, I11 remove "using the vortex average method"

Response: Done.

Reviewer comment: I14 a simpler reference date in January would make comparisons with other data sets easier.

Response: We partly agree with the reviewer, but decided to leave this as is, because the start date does not affect the main results of this study. We also calculated the chemical ozone loss rate, which can be used for comparisons with other data sets.

Reviewer comment: I15 describe situation in all winters more comprehensively (for example in comparison with findings of previous studies, see specific remarks)

Response: The discussion section, particularly in terms of comparisons with other studies (both hemispheres) was significantly extended and improved.

Reviewer comment: I17-I24 belongs rather in the introduction (more comprehensive description of chlorine induced ozone loss).

Response: We agree that these two sentences don't fit here and removed them.

Reviewer comment: I24-29 Did SCIAMACHY observe's above 20km?

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Response: Yes, but we studied here only the 16-22 km altitude range (450-600 K) where large ozone losses occur.

Reviewer comment: I10 "ozone mass loss for a certain isentrope range": Which isentrope range? How is the ozone mass loss derived for such an isentrope range? This needs some further explanation or rewording.

Response: With "certain isentrope range" we meant 450–600 K. This part of the text was reworded in the new version of the manuscript.

Reviewer comment: I25 Mixing of air inside and outside the vortex can also happen before March, this is highly variable between different years.

Response: This point is now mentioned in the new version of the manuscript.

Reviewer comment: p6571, I16 remove " a vortex average method"

Response: Done.

Reviewer comment: p6572, I2 remove "employed here"

Response: Done.

Reviewer comment: Use of "ozone losses" / "ozone loss": check usage throughout paper.

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Response: Done.

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