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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.

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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 referee 1: Simone Tilmes

12 October 2011

General comments

Reviewer comment: Chemical ozone loss in the polar vortex in both hemispheres is derived based on SCIAMACHY satellite data using the vortex average approach. This paper investigates 7 winters between 2002 and 2009. Results from the vortex average approach in this study are compared to results form other studies for the Arctic winter 2005. The paper as currently presented leaves many open questions with regard to the performance of the method, as well as the purpose of some analysis of this study. Therefore I recommend a major revision of this paper before it should be considered for the publication in ACP.

Response: We thank the reviewer for her detailed and thoughtful comments. We followed most of the suggestions made by the reviewer and we think that the manuscript has significantly improved after the comments and suggestions have been considered. Please find our detailed responses to the points raised below.

Reviewer comment: The Method: It is not clear from this paper how ozone loss derived here is reasonable in comparison to results from other studies in Arctic and Antarctica. All we learned is that the method results in reasonable ozone loss values in March 2005.

Response: This is a valid point, and in response to it we significantly expanded section 5 dealing with the comparison of chemical ozone losses derived in our paper with other studies (for both hemispheres). Furthermore, responding to the points raised by reviewer #2 we comprehensively revised the method section and added a step by step explanation of the methodology.

Reviewer comment: Various references to other studies are missing.

Response: The number of references dealing with chemical ozone losses on both vortices was markedly increased, also following the suggestions made by the reviewer.

Reviewer comment: Scientific purpose of the results shown in Figures 13 and 14 is not clear.

Response: We showed Figure 13 and 14 to illustrate the relationship of polar chemical ozone loss, PSC occurrence rate and QBO signature from our results in more detail. Please see our responses to the specific comments below, that address these points in more detail. We also added additional text to the discussion section to better

motivate these Figures.

Specific comments

Reviewer comment: Introduction: Line 9: Processes that cause in Arctic and Antarctic ozone loss are for the most part understood, but there are still uncertainties that need to be investigated. For example models are not able to reproduce chemical ozone depletion (see SPARC 2010, Chapter 6 and references therein).

Response: Thank you, this point is added in the introduction.

Reviewer comment: Line 13: Antarctic ozone loss was shown to cover a range of 350-600 K, as for example discussed in Tilmes et al., 2006 (JGR)).

Response: Our statement focused on the altitude range with 'near-total' ozone destruction, and we still think this statement is not incorrect. But we did add a note saying that ozone losses are observed down to the 350 K isentropic level and included a reference to the paper suggested.

Reviewer comment: Line 19. There are certainly more studies that could be added, see WMO 2006, Chapter 4.1.2.2 and references therein.

Response: Done.

Reviewer comment: Section 2.1: Is SCIAMACHY ozone evaluated against other independent observations? Explain the vertical coverage of the instrument here and why ozone loss is only derived between 450 and 600K and not further down.

Response: Different versions of ozone profile data retrieved from SCIAMACHY limb scatter observations have been compared with essentially all available satellite data sets (e.g., SAGE II, HALOE, SABER, ACE-FTS MLS) and the agreement is generally within 10% in the stratosphere. The best agreement is usually found with SAGE II solar occultation instruments. For the data version used here (2.0) comprehensive comparisons were done with SAGE II, but unfortunately not published. A validation study of a slightly improved version of the data product – also showing agreement generally within 10% (in the stratosphere) with all available satellite sensors – has just been submitted to Atmospheric Measurement Techniques (Mieruch et al., Global and long-term comparison of SCIAMACHY limb ozone profiles with correlative satellite data (2002-2008), AMT, submitted).

Although there is not really a hard threshold of the lower sensitivity range limit, the retrieval becomes more and more insensitive to atmospheric ozone below about 15 km – because the instrument's line of sight becomes optically thick. This is the reason, why 450 K is the lowest isentropic level studied here. The retrieval altitude range was already mentioned in the paper, and we added the vertical resolution (about 4 – 4.5 km) of the ozone profiles.

Section 2.1 was extended by the information summarized here.

Reviewer comment: Section 3: The area of the polar vortex is often defined using equivalent latitudes, as for example described in Mueller et al., 2008, and references therein. Consider plotting Figures 1 and 2 with regard to equivalent latitudes and discuss how your definition compared to the one given in Mueller et al., 2008. Figure 4 could be also plotted with regard to equivalent latitudes.

Response: Although this is a very good suggestion, we believe it would not impact the main findings in this study. Therefore, we decided to leave the paper as is in this respect and focus on the other aspects to improve the manuscript. We will consider this in the future.

Reviewer comment: Section 4: Page 6564: The calculation of the ozone changes due to vertical transport is the main factor that controls the amount of ozone loss in this study. Therefore it is very important to understand whether this calculation is valid. To prove this, you might want to consider looking at a long-lived tracer in the lower stratosphere (like N2O) and apply your method. If your radiative heating calculation is correct, the derived change of N2O should be in agreement with the observed. In this way, you might be able to estimate the uncertainty of this calculation.

Response: This is a really good suggestion, but we believe that this investigation is beyond the scope of the current paper. The vortex averaging method used here is - despite some weaknesses - an established technique to estimate chemical ozone losses. Furthermore, the scheme to determine the heating rates is a standard and often used scheme, and has also previously been used to calculate diabatic descent rates (e.g., Eichmann et al.). Last not least, our derived chemical ozone losses are in good agreement with other studies (see the significantly improved section 5 on comparisons), indirectly verifying that the calculated descent rates cannot be entirely unrealistic.

Reviewer comment: Figure 3: Instead of the error bars, shading might help to illustrate the uncertainty of the distribution

Response: Done.

Reviewer comment: Page 6565: line 4: change ‘seasons’ to ‘years’

Response: Done.

Reviewer comment: Line 11: what does ‘first spike’ mean?

Response: It means the first maximum of the observed ozone and we changed ‘spike’ to ‘maximum’ in order to avoid any confusion.

Reviewer comment: Describe how you determine the standard deviations in Figure 3, a and b, and what they mean. Also, the lines in panel b are hard to read. Consider limiting y-axis range. Page 6566: Line 6ff. This comment goes back to the early comment about the performance of the method to derive chemical ozone loss. The resulting chemical loss is very variable in Figure 3,d. Vortex average ozone loss is expected to not decrease during the ozone loss season, because there is no production expected and vortex internal mixing process are not significant. What causes this variability? Are there problems in deriving the amount of decent of the vortex?

Response: 1: The determination of the standard deviations in Fig. 3 has been described in Section 4.1. For panel a) it corresponds to the standard deviation of the individual SCIAMACHY ozone measurements inside the polar vortex about the daily mean value. In terms of panel b) it is the propagated standard deviation originating from the standard deviation in panel a). The solid lines show the daily averaged values.

Response: 2: The y-axis range of panel b) was adjusted as suggested.

Response: 3: Regarding the variability in Fig. 3d the origin for these effects are not fully established but it is likely related to the neglected transport across the vortex boundary as well as horizontal inhomogeneities inside the polar vortex in combination with the fact that SCIAMACHY does not allow sampling of the entire polar vortex. We added some text discussing this. See also responses to comments by reviewer 2.

Reviewer comment: Further, to derive the ozone loss, how do you justify that a linear fit is reasonable over three months? Ozone loss rates accelerate with increasing sunlight spring. Ozone loss rates can be for example given for single months.

Response: Thank you, this actually is a valid point, and the information content of a linear fit is not clear. The linear fit and the discussion of the corresponding results has been deleted in the revised version of the manuscript.

Reviewer comment: Line 20. How do you justify the large ozone loss values in 2009? Did the polar vortex move towards low latitudes receiving more sunlight in early winter? This is however not very obvious from Figure 1, bottom panel. Are there other studies showing this? It is not enough for ozone depletion to have cold conditions in early winter, you also need some sunlight to start the catalytic cycles.

Response: The main reason for the large ozone loss rates is the fact, that the ozone loss got interrupted by the major warming in mid-February. Therefore, the ozone loss rates are based on the the early phase of the ozone loss period, where daily ozone loss rates are larger than average (as, e.g. seen in fig. 3e). Fig. 3 also indicates that the loss rates in early 2009 were not anomalous with respect to the other years with significant catalytic ozone losses (2005 and 2007). We cannot exclude that geophysical effect – as suggested by the reviewer above – may contribute to the large ozone losses in early 2009, but the results presented in our study are consistent with Kuttippurath et al. (2010). Discussion on these aspects was added to section 4.1.

Reviewer comment: Line 25: please add at the 475 K potential temperature level.

Response: The SSW event in early 2006 did certainly not only affect the 475 K level but also the other levels studied. Therefore the statement was not not only limited to the 475 K level here.

Reviewer comment: Page 6566: How do your results compare to earlier studies for other years than 2005 (for example studies mentioned in the WMO report, as well as Kuttippurath et al., 2010, Harris et al., 2010, Tilmes et al., 2004 for the first two years considered).

Response: Section 5, dealing with comparisons of the derived ozone losses with other studies was significantly expanded. It now includes comparisons for the Arctic (section 5.1) and the Antarctic (section 5.2) polar winter/spring seasons.

Reviewer comment: Page 6567: Line2-8: Winters with high ozone loss 05,07, and 08, and not consecutive years. . .. It is not clear what the authors mean here.

Response: We don't fully understand the reviewers comment, as the sentences in

lines cited 2–8 on page 6567 seem to be very clear. We're stating that the highest PSC occurrence rates in February occurred in the years 2005, 2007 and 2008. We further state, that the alternating behavior from 2003 to 2007 is consistent with the (statistical) QBO dependence of the vortex stability etc. This effect (as discussed in the works by K. Labitzke and e.g., Camp and Tung, JAS (2007)) is of statistical nature and cannot be expected to hold for every winter.

Reviewer comment: Section 4.2: Can you add a comparison of your results to early studies? Antarctic ozone loss was comprehensively discussed for the winter 2003 in Tilmes et al., 2006, using ILAS satellite observations. Huck et al, 2006, performed a multi-year analysis of ozone loss over Antarctica. There are other studies as well.

Response: A discussion of the agreement of the chemical ozone losses in the Antarctic polar vortex with other studies was now included as section 5.2.

Reviewer comment: Section 5: As mentioned above, add comparisons looking at other winters in Arctic and Antarctica as well.

Response:: Section 5 was significantly extended. In section 5.1 we now compare the Arctic ozone losses with the results published in other studies. Section 5.2 focused on the southern hemisphere.

Reviewer comment: Figure 7, and corresponding text as well as Figure 13 and 14 and the Discussion: The authors calculate the O_3 mass loss and the daily mean of the vortex volume. These two measures do not correlate very well, considering both Arctic and Antarctica. What is the point the authors trying to make here? Also, in Figure 13, a relationship between PSC occurrence rate for February and ozone loss seems not to make much sense, since ozone loss till April is accumulated over the entire winter.

Response: We partly agree with the reviewer. There is certainly not a perfect correlation between the vortex volume and the ozone mass loss. However, the values are not entirely uncorrelated, either. The Antarctic points (red) do not show large interannual variability, as expected, but taken all seasons in both hemisphere as a whole shows that a linear fit of the vortex volume and the ozone mass loss (for the studied range of potential temperatures) is not a bad approximation.

Regarding Fig. 13 and 14 we disagree with the referee. Perhaps our interpretation of these dependences was not well explained. We're certainly not claiming that PSCs in February only are responsible for the entire ozone loss. Please see our response to the next point for more information. Beside the apparent correlation between PSC occurrence in February and the chemical ozone loss by April 1 a main purpose of Fig. 14 is to illustrate the QBO impact on these quantities. Our relatively short time series are certainly not sufficient to prove a QBO effect, but the analysis by Camp and Tung, JAS (2007) and the many papers by K. Labitzke and co-authors demonstrate that - during solar minimum conditions - there is a statistical QBO effect on the stability of the Arctic polar vortex, e.g., the February mean north pole temperatures, and consequently on PSC occurrence and also the chemical ozone loss.

Reviewer comment: Do you suggest, that only the PSC occurrence in February matters for the chemical ozone loss in the entire winter? How do you justify this? There are many studies showing significant ozone loss in the early winter (in January).

Response: This is an important point, and the reviewer is right, that we didn't explain this well. We're certainly not claiming that PSCs occurring in February are responsible

for most the chlorine activation leading to the observed ozone losses. The interpretation is rather the following: If a major warming occurs (typically in January or early February), then the ozone loss is stopped, the polar lower stratospheric temperatures increase, leading typically to the disappearance of PSCs. This is our explanation for this apparent correlation between the chemical ozone losses and the PSC occurrence in February. This is now better explained in the paper. Note that the PSC occurrence rates in January do not show this strong inter-annual variability. However, PSC occurrence rates for January were also included in the new manuscript.

Reviewer comment: Rex et al., 2006, Harris et al., 2010, discuss the relationship between O3 loss and the volume, where PSC potentially exists. Also, Tilmes et al, 2006b, introduced a correlation between O3loss to a PSC formation potential (however only till 2005), which describes the fraction of the vortex that is potentially covered. There, also the volume of the vortex for different winters was calculated.
The authors might consider correlating the PSC occurrence rate in this paper for the entire season, with the PSC volume derived in the studies mentioned above.

Response: That is another good idea. However, with SCIAMACHY limb scatter observations we are not able to determine PSC occurrence rate for the entire season, because sunlight is required for the PSC detection as explained in section 2.2. Instead we're now investigating the correlation between PSC occurrence rate in February and the PSC volume V_{PSC} for the entire season. The V_{PSC} from WMO 2011 was used for this purpose and this time series was compared to the polar vortex volume and PSC occurrence rate determined in this study, as shown in the new Figure 7. Note, that our intention of using PSC occurrence rates here is of limited nature. PSC occurrence is simultaneously observed with SCIAMACHY limb scattering observations and available for analysis. However, we realize that cloud occurrence is not the most objective physical variable.

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