REPLY TO REFEREE 2 OF acp-2010-343

We thank the referee for his/her valuable review of this article.

The paper presents a comprehensive overview over Arctic stratospheric ozone losses for the most recent **SIX** winters. This makes the paper a valuable contribution although nothing really new is reported,

 # Contribution of various chemical cycles to the ozone loss in the polar stratosphere in different climatic conditions spanning six years is something new to offer, as the available studies are dealing with mid-winter warming and associated (mid-latitude) ozone loss. Konopka et al. 2007, is a kind of exception to this, but they used a box model instead of a 3-D CTM as we do. This is what motivated us to perfom the study.

The paper should clearly been published in ACP after some revisions.

 # Thank you for recommending the paper to be published in ACP.

page 14678, line 15: Do the particles sediment and if so, how?

 # The model has a detailed scheme of PSC formation and growth. The saturation vapour pressure given by Hanson and Mauersberger (1988) is applied to assume the existence of NAT particles and Murray (1967) for water-ice particles. A denitrification scheme is introduced to account for the sedimentation of HNO_3 containing particles where the NAT particles are assumed to be in equilibrium with gas-phase HNO_3. All the three types of particles –NAT, Ice, and Liquid Aerosols – are considered for the sedimentation. The sedimentation speed of particles is estimated according to Pruppacher and Klett (1997). A constant number density ($5e^{-3}$ cm^{-3}) of NAT particles is considered for the analysis (Waibel et al., 1999).

page 14680, line 26: To me the peak loss appears to be substantially larger in 2005 (dark red color, exceeding 1.6 ppm) than that in 2010.

 # Corrected. Please find the revised Sec. 5.2.2, Paragraph 2, Lines 6-10. Please note that there are some changes in the ozone loss values after the bias correction.

page 14681, line 14: Any bias between ECMWF ozone and MLS ozone will show up as ozone loss or production in this calculation. If the differences are negligible the analysis is fine but that has to be shown. If the differences are not negligible a correction needs to be applied.

 # There are some biases in intial days, which are more than 1% as expected. This bias has been corrected for the revised version. Please find the new Figure 4 and its discussion in Sec. 5.2.2

page 14681, line 21: The "large losses" in the observations are partly caused by the bias issue described above. This is particularly pronounced in 2010 and that explains why the model / observation differences are so large for that winter:

 # True. Since the difference in ozone is relatively large as compared to other years, the “observed” ozone loss is also slightly large. In addition, the tracer values are higher than that of other years, which even widened the difference (Tracer - MLS ozone). Further, if we look closely the ClO comparisons, its clear that the simulated ClO is lower, and hence, chlorine activation in the model is not well captured.

page 14682, line 8: Since the absolute differences in ozone, particularly those right at the beginning of the calculation, are really important, a plot of the differences should be shown.

 # Done. Please find the new Figure 2.

Sections 5.2.4. and 6.3.1 I generally agree with the comments by Jens-Uwe Grooss about comparisons between ozone loss results from various techniques. But Doing a thorough comparison with any one of the previous studies here would go beyond the scope of the paper and would be a study of its own. I think clearly mentioning the caveats and otherwise leave the comparisons unchanged should be sufficient. I just can't see any other practical solution here.

 # We also feel that an extensive discussion on just one winter in such a scale would go beyond the scope and aim of the paper. Still, we have provided a few additional calculations relevant to this discussion. Please find the revised Sec.6.3, and Table 2 and Table 3.

Page 14686, lines 10-15: Comparing ozone loss rates in terms of loss per sunlit hour is very tricky. It depends a lot on using exactly the same definition for what a "sunlit hour" is. Stricter definitions (e.g. those based on a smaller sza cutoff) result in substantially larger ozone loss rates. Changing the cutoff from 95 deg to 90 deg can change the derived ozone loss rates in terms of these different sunlit hours by nearly a factor of two under some conditions. So some words should be said on the definition of a sunlit hour in this work and care should be taken that this definition is in agreement with that used in Frieler et al..

 # Done. A good point. We use a different criterion for sunlit hour calculation and is given in Sec. 6.1. Therefore, a qualitative comparison is presented.

Page 14690, lines 2-7: Hm, since the ClO-O cycle (which is a halogen catalyzed cycle) is the second most important cycle at these altitudes and contributes up to 20-55% I think saying that "the halogen catalysed cycles play little role" is somewhat misleading.

 # We have reformulated "little role" to "comparatively a small role". Please find Sec. 6.2.2, Line 2

Page 14691, line 16: Calling 350-850K the total column is a bit sloppy.

 # Noted. Changed to "partial column". Please find the revised Sec.6.3, and Table 2 and Table 3.

Table 3: The date for which the estimate is given should also be given in the table, particularly because it is so different for the von Hobe et al. estimate. Also major deviations in the domain and time covered could perhaps be printed in bold or italics, to make reasons for deviating loss estimates more obvious.

Done. Table is modified as suggested. Please find the revised Sec.6.3 and Table 3.

Page 14694, line 7: I can't see the basis for this statement. Feng et al. reported larger losses for the partial column. Also Rex et al. note that in 2005 a substantial part of the column loss took place in the lowest part of the 350-550K region, i.e. below 400K and also significant contributions from below 380K. That region is not included in the studies that report smaller values for the partial column, with the only exception of the present work.

 # Noted. The section is slightly modified with additional calculations and information. In fact, we wanted to mention the measured loss from different estimates. This is why the loss estimate from Feng et al. (2007), which is larger than that of Rex et al (2006), did not consider while making the statement. We have reformulated the sentence. Please find the revised Sec.6.3.1, Paragraph 1.

REFERENCE:

Murray, F.W.: On the computation of saturation vapour pressure. J. Appl. Meterol. 6, 203–204, 1967.
Pruppacher, H.R., Klett, J.D.: Microstructure of atmospheric clouds and precipitations, 2nd ed. Kluwer Acad. Publishers, 1997.
Waibel, A.E., et al.: Arctic ozone loss due to denitrification. Science 283, 2064–2069, 1999.