REPLY TO REFEREE 1 OF acp-2010-343

We thank the referee for his/her thorough review of the paper.

General:
This paper is a valuable contribution to the ongoing effort of monitoring and characterising Arctic polar ozone loss using models and observations. The paper is suitable for ACP and should be accepted after some revisions, as indicated in the detailed assessment below.

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

Point-by-point:
p14676, l23-24: This sentence is an oversimplification; provide more detail (see below).

 # Done. Please find the revised Sec.1, Paragraphs 1 and 2.

p14677, l10: Not sure this is entirely true, but you should at least provide more details and explain what you are adding to the discussion.

 # Done. Sec. 1 is modified with relevant information. Please find the revised Paragraph 2.

p14678, l3-4: What does climatological H2O and CO2 mean? Not the values used or provided in ECMWF?

 # No, these are the climatologies used for the calculations in MIDRAD, as described in Shine (1987).

p14680, l3: Not sure what "tangibly illustrate" means. Is there anything more to it than pointing out that interannual variability is large?

 # We state that the inter-annual variability is clearly shown by the maps. Alternately, the maps also show the model's ability to capture different dynamical features related to transport in that detail. We have replaced "tangibly" with "clearly" to avoid confusions. Please find the revised Sec. 5.2.1, Paragraph 2, Line 14.

p14680, l10-13: You should add a little bit more about the meteorology, in conjunction with improving your vague statement at p14676,l23-24)! … a few sentences putting the years into context – you do this a little in section 4, so you could expand there and refer back to it.

 # We have modified Sec.1 . Please find the revised Paragraphs 1 and 2. As suggested, in order to discuss the ozone loss variability in previous Arctic winters, we have referred back to it.

p14681, l11: Diagnosing ozone loss using MLS observations and the passive tracer in the model seems to be a reasonable approach, but for my feeling you tell the story the wrong way round..... I feel it would be appropriate to swap figures after figure 1: first figure 4, followed by figure 2 and 3, and the structure of the text should be adapted accordingly.

 # We have changed the order of Subsections as suggested. Now ozone simulations and comparisons are presented first (Sec. 5.1) and then the ozone loss (Sec. 5.2.1 and 5.2.2), and ClO comparisons (Sec. 5.2.3).

In addition, error sources for the estimate should be addressed more clearly:

 # We understand the importance of error estimation. However, it would be very difficult to quantify errors in the model run results. Factors such as inaccuracies in model transport and high latitude descent are some examples. Further, uncertainties in the meteorological files used for forcing the model could also contribute to "total possible bias" in the model results. Estimation of bias from comparison with a single data set (here MLS) cannot be regarded as a representative "total error"

in our loss estimates. Though it is possible to derive standard error from standard deviation of ozone loss inside the vortex, we suspect that it will not adequately represent the "error" for the ozone loss estimates. Therefore, we haven't attempted to quantify the error for the ozone loss estimates here.

p14682, l1: The paragraph starting here should come earlier and should link closely to the discussion of the initial conditions for ozone.

 # Done. The order of the section is changed as suggested. Now it is Sec. 5.1.

p14684, l23: It is good to cite Jackson and Orsolini (2008), but the reader should be reminded what their findings were.

 # Done. Apart from introducing a new technique using assimilated ozone fields that slightly different from Rosevall et al. (2007, 2008), the article presents ins and outs of different ozone loss estimation methods. Therefore, we also cite the article instead of reminding all the points noted in it.

Section 6 (first part): I feel that the first part of the section does not add much with quite a large number of words. It would be a good strategy to shorten the section and sharpen it up - I have no strong feelings about it - but the authors should consider this suggestion.

 # We have tried to shorten the section. However, as per Referee 2's suggestion, we have included the sunlit hour calculation and its description in the revised version. Please find the new Sec. 6.1.

Section 6.3: I got quite confused here: Basically you are talking about different partial column losses. Because one column has a larger vertical extent you identify this column with a total column. I don't think I agree! I would suggest you refer to all of them as partial columns (this is what they are – none of them is a column as defined by e.g. TOMS or SAOZ). This should be reflected in table 2.

 # We have changed both to partial columns in the text and tables. Please find the new Sec. 6.3 and, Table 2 and Table 3.

In table 3 many different partial columns are compared: Interestingly you have one of the largest disagreements with the study that uses the same height regime. I think some more explanation is required here. You might even consider to calculate different partial columns from your model (e.g. 380 K to 580 K).

 # We have calculated different ranges and are discussed in the text. To compare with Grooss et al., we have calculated the loss at 375-550 K range (model's next vertical level is 600 K). Please find the additional calculations, modified text and tables in Sec. 6.3.

What is the error/uncertainty of your assessment?

 # If we consider the spread of ozone loss inside the vortex, the measurement accuracies and the initial bias in simulations, and take its root sum square, then it is about 9.6%. However, we do not want to call this as the error of our estimate, since a more detailed comparative study with different measurement clusters are needed to provide a solid "error estimate" for the ozone loss analysis.

In addition, I would prefer a structure in which the earliest year (including the intercomparison of ozone losses) is discussed first and later years follow in order.

 # This section in fact is organised in line with the ozone loss discussion based on mixing ratios (Sec. 5.2 on ozone loss), where salient features are mentioned instead of a chronological description, to avoid repetition and to further focus on our points. This is why we kept "6.3.1. Ozone loss in 2005" in the end since we want to present our estimations (Mimosa-chim, MLS and UV-VIS) first before making a comparison with other studies. Therefore, to be consistent with our previous discussions, we prefer to present the section as it is. Additionally, Referee 2 has no such suggestion, but recommends to keep it as it is. We hope that the Referee 1 will find it as an appropriate decision.