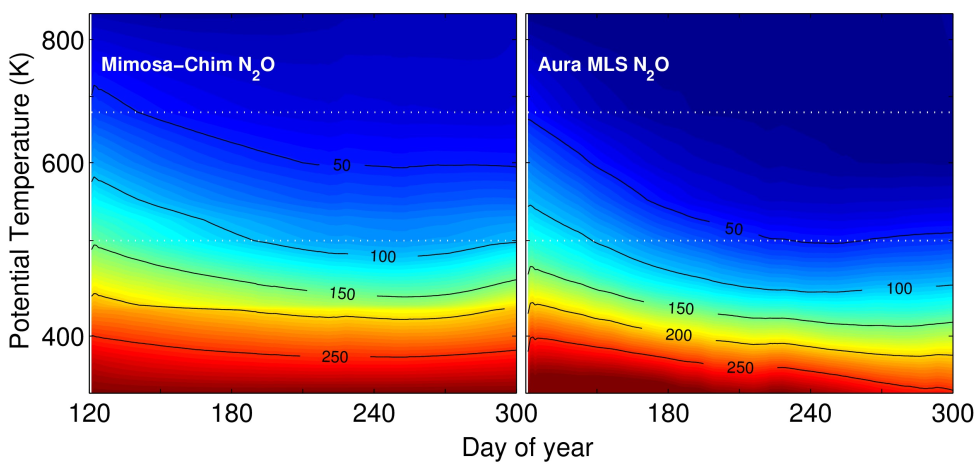
REPLY TO REFEREE #1 COMMENTS

The paper can be published in ACP but it requires some clarification. Specific comments can be found below:

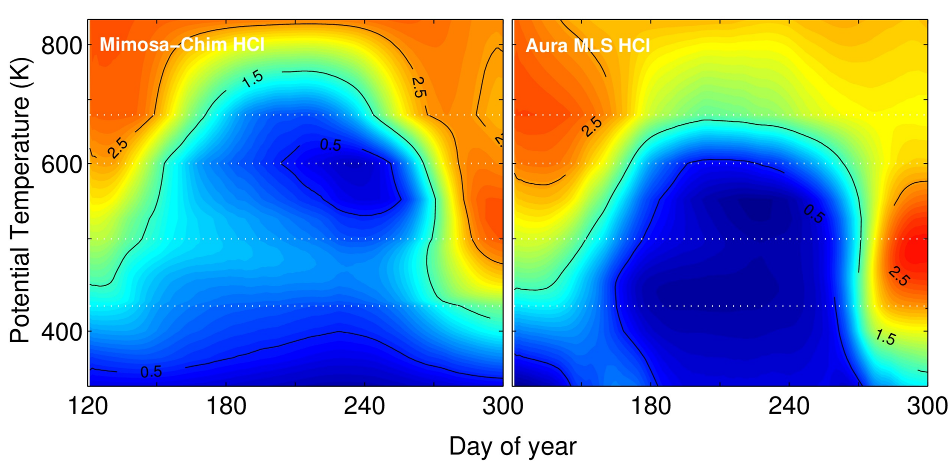
Thank you very much for your time and evaluating the manuscript. Please find the revised manuscript and the answers for your questions and comments below.

Specific comments: 1) Chlorine activation. The model has too low ClO compared with MLS data. It is not convincing to conclude "largely due to the slower vertical descent in the model during spring". Why are there two layers of the modelled HCl in Figure 4 (almost constant HCl values 0.7-1.2 ppbv around 500 K)?

These statements (lines 353—377) are based on different tests performed with the model. The conclusion about the slower descent was made from the analyses based on the dynamical tracer, N2O (see below). It is shown that the simulated N2O values are higher than the measurements at all altitudes. The dynamical descent in winter/spring stratosphere is clearly illustrated with the measured N2O fields, but it is evidently slower in the model. Because of the slower descent, the HCl values are relatively smaller in the simulations in the lower stratosphere in early and middle winter and in late spring (e.g. after Day 260, see 2.5 ppbv contour in the measurements) and hence, the activated ClO is smaller too.



We think that the small features in the HCl plot were not clear in Figure 4. Please find a bigger plot here. In fact, the measurements also show a similar feature of relatively higher values around 500 K (see the very small value pools of about 0.2—0.3 ppbv above and below 500 K in the measurements). As mentioned in the text, the chlorine partitioning is not very effective in the model, which also constitutes the higher HCl values in the simulations and that make the difference more pronounced in the simulations.



2) Ozone loss. How good is the modelled ozone compared with MLS for Antarctic winters 2004-2013? This should be the first step to check the model and it is important to understand the ozone loss difference between model and MLS.

We have presented the measured and modelled ozone in Figure 4. Comparisons made from the average (of ten winters) reveal that the differences in May are negligible at all altitudes. In June—August, the model underestimates the measured ozone by about 0.2—0.5 ppmv below 500 K. In September and October, the model slightly overestimates (0.2—0.4 ppmv) the measured ozone below 500 K, with a peak around 400 K. Above 500 K, the model underestimates the measured ozone from about 0.2 ppmv in June to 0.5—1 ppmv in August/October. These differences are also qualitatively consistent with the slower vertical descent in the model. This has been mentioned in lines 366—377.

Form Figure 1, it looks like that the ozone loss between 65-67S equivalent latitude (EqL) is much larger than the value inside the polar vortex in September and October. Why is it? Can you explain why the diagnosed ozone loss for October from MLS has similar distribution and value in Figure 1? I think the air mass should be different for each EqL region.

Please note that the region 65—67 S equivalent latitude (EqL) is still inside the vortex, but close to the vortex edge. The sunlight reaches this region before the higher EqL regions. Therefore, the ozone loss starts first in this region and hence, the monthly average accumulated ozone loss is higher there.

In October, as the ozone loss cycles stop, mixing processes affecting the air masses within the vortex homogenise ozone concentrations. This is reflected in the ozone loss values and has been mentioned in lines 245—254.

3) It is not clear which ECMWF analyses data used to force the model. Is it operational data or ERA-Interim reanalyses? If ECMWF operational analyses are used, it needs to mention that the ECMWF vertical levels has been changed to 91 levels from Feb 2006 and to 137 levels from June 2013.

We have used the ECMF operational data, as mentioned in lines 126—131. The change in the vertical levels of the data is also noted there.

4) Initialisation. It reads weird that the chemical fields in the model are initialized from a different CTM (REPROBUS). Does this mean that the Mimosa-Chim needs ten years initialisation of chemical fields from REPROBUS simulations? Not sure how good the intialisation compared with available MLS data on 1 May. Is it possible to get the initialisation from even a low or standard Mimosa-Chim simulations?

We have initialised the Mimosa-Chim model simulations from a long-term simulation of the REROBUS CTM. This model is equivalent to a lower resolution (2 X 2) version of the former model. The simulations are generally in good agreement with the measurements. It has to be noted that the REPROBUS simulations are also driven by the same ECMWF analyses (used for forcing the Mimosa-Chim CTM). This has been mentioned in lines 131—136.

5) Why have not they sampled the model output at MLS locations and time? These should be easily done by doing the interpolation during the model simulations.

This is what we have done for comparisons, and is noted in lines 177—178/figure captions.

6) It would be good if the author can add one section to describe the meteorological conditions for these ten years (e.g., temperature, vortex area, PSC surface area).

This has been done. Please find the new Section 3.2.1.

Minor comments: 1) Abstract, lines 3-4, why "high frequency polar vortex observations"?

As mentioned in lines 5—9 and 80—109, this is the first time that the southern polar vortex region is observed with such high resolution observations for a range of winters. Therefore, the simulations compared to observations give an opportunity to analyse the polar processes in these different winters in a better way (than that did in the past with relatively coarse resolution observations).

2) Abstract, line 7, maybe change "69" to "67" which will be consistent with the text in Page 28208 Line 2 etc.

Done. Please find it in line 12.

3) Introduction, Page 28205 Line 27, add a reference for UARS MLS there.

Done. Please find it in line 102.

4)Page 28206 Line 3, add a reference for Mimosa-Chim model.

Done. Please find it in line 111.

5) Page 28209 Line 9, change "is" to "are".

The "is" stands for the "assessment".

6) Page 28208, Line 24, "see later analyses", do you mean sections 3.2 and 3.3.4?

Yes, this has been mentioned in line 247.

7)Page 28209 Line 1, Can you explain why the modelled and measured O3 is so large in September?

We hope the referee mentions ozone loss, not ozone. Please find the explanation in lines 245—254 and 366—377.

8) Page 28209 Line 15, In Figure 2, it is clearly seen the value is over 0.3 ppbv/sh from BrO-ClO, why is this cycle "hardly exceeds 0.3 ppbv/sh for August and September.

This section has been removed. Please find the reply to Referee #2 comments.

9) page 28211 Line 13, why the model overestimates the denification?

This is related to the Equilibrium PSC scheme. This has been mentioned in lines 385—389.

10) Page 28211 Line 19, add a reference for SLIMCAT.

Done. Please find it in line 400.

11)Page 28211 Lines 28-29, it seems this is the problem in Minosa-Chim CTM, but why is "still a critical issue" in "most CTMs"?

This sentence has been removed.

12)Page 28213 Line 10, add a reference for SCIMACHY.

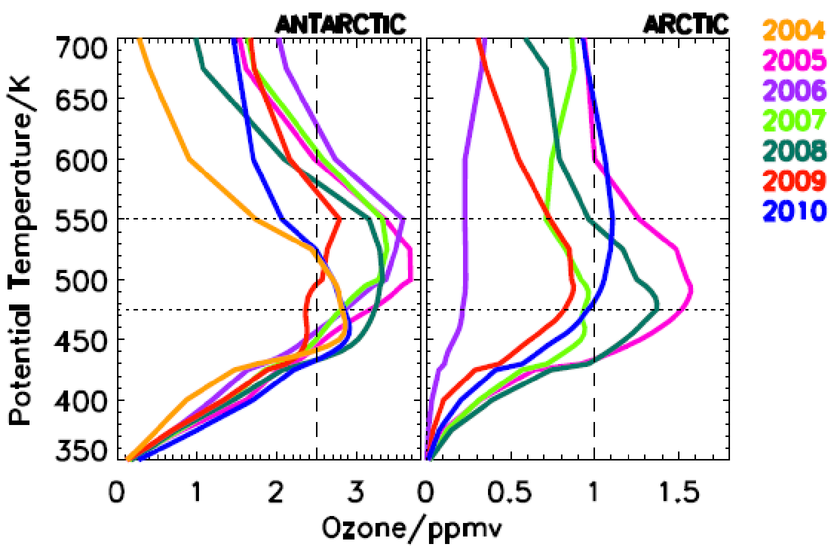
Done. Please find it in line 472.

13) Page 28213 Line 22, change "consistent with" to "depending on"

Done. Please find it in line 497.

14) Page 28214 Line 25, is it "around 550 K"?But from Figure 5, the peak loss altitude is below 500 K.

The peak is found above 500 K, and mostly in the 525—550 K region. For instance, we have checked this for some winters and is shown below. The sentence has been modified in line 542.



15) Page 28215 Line 14, define the vortex core.

Inside the vortex at higher EqLs, e.g at 70—90 S EqLs. This has been mentioned in lines 566—567.

16) Error bar for MLS in Table 2 should be estimated.

Done. This has been mentioned In Figure 6 caption.