Response to Anonymous Referee #2: Interactive comment on “Total depletion of ozone reached in the 2010–2011 Arctic winter as observed by MIPAS/ENVISAT using a 2-D tomographic approach” by E. Arnone et al.

A) General comments

The Paper of Arnone et al. is the third one with respect to the ozone depletion in the Arctic in winter/spring 2010/2011 (see Manney et al. 2011 and Sinnhuber et al. 2011). The presented data set from 1 December 2010 till 15 April 2011 is nevertheless unique (e.g. article Manney: MLS data gap from 27 March till 20 April and only a few PSC data shown; article Sinnhuber: no PSC data). As in the Manney article Arnone et al. concentrate an measured data sets and interprete these measurements in comparison to measured data during the years before, Sinnhuber et al. intercompare measured data of 2010/2011 with corresponding CTM calculations. The main results of the three articles are the same; a long-lasting Arctic vortex in 2011 with persistent cold temperatures, an obvious denitrification of the vortex and extraordinary ozone loss in spring 2011 (something like the first “ozone hole” in the Arctic). On the other hand there are also differences in the content of the three papers. MIPAS data are available up to the North Pole and cover therefore the vortex very well. Arnone et al. use a 2 D retrieval which takes best into account the strong horizontal inhomogeneity of the atmosphere during polar winter. They also have determined the PSC distribution and its temporal development for the whole Arctic winter. Looking into detail of ozone loss there are also some discrepancies between the three papers which should be explained as far as possible. Concluding the general comments, the paper of Arnone et al. deserves to be published in ACP but the authors should consider the following detailed comments and improve the paper.

We are grateful to the reviewer for the work in assessing our manuscript. We were happy to follow the reviewer’s comments and improve the manuscript as suggested. Indeed the newly published work by Sinnhuber et al. deserved some comments, and we were happy to add a comment on their analysis and point out to the difference in the minimum ozone values obtained by their and our analysis. Detailed answers are given here below. In some cases, changes to the manuscript were applied following the other reviewers’ comments, and extended answers are given in the other replies as specified.

B) Specific comments:

1. Title of the paper

The title should be changed because total depletion of ozone may be accomplished only in atmospheric layers, if at all (see comments below). Proposal: “Extreme ozone depletion…”

The title indeed intended to point to the peculiarity of the zero-ozone measurements at 18 km altitude found in the MIPAS2D dataset. To avoid misleading the reader we changed the title to “Extreme ozone depletion in the 2010-2011 Arctic winter stratosphere as observed by MIPAS/ENVISAT using a 2D tomographic approach”

2. Sect. 2.1, page 33195 In principle, MIPAS is able to collect measurements from pole to pole. The actual tangent points depend on the time of measurements. Delete “89.3°and 87,5°” and replace it by “MIPAS to measure from pole to pole”.

The revised text was updated as suggested.

3. Sect. 2.2, page 33196, “… the distance of MIPAS tangent points from their along track projection (up to 2° latitude)”. Not true e.g. in case of a tangent point at 89,3°N !

We agree the sentence was misleading. In the geofit approach, one parameter is retrieved from all lines of sight contributing to the portion of atmosphere at a given geolocation. Since MIPAS lines of sight deviate from the satellite orbit plane to attempt to follow a meridian, there is no ideal overlap of all lines of sight. Taken a geolocation, an assumption is made of across track homogeneity for those lines of sight that pass through the portion of the atmosphere of the target geolocation. The 2 degree we referred to is the maximum latitudinal component of the deviation of the lines of sight contributing to the analyzed portion of the atmosphere from a perfect alignment. What matters is the difference in alignment of the lines of sight between a few adjacent scans, which is relatively small since successive scans are recorded at a similar azimuth of the pointing direction. We have revised the sentence to clarify the meaning of the maximum 2 degree latitudinal component of the across track homogeneity assumption.

4. Sect. 3.1, page 33201: Fig 1/2: Why are there so many small gaps in the time series of temperature and trace gas concentrations? An explanation is expected.

We agree the small gaps in the time series make the plots less readable. In the original manuscript we used only measurements from the nominal observation mode. This implies that measurements from the middle atmosphere or upper atmosphere observations modes were missing, i.e. 1 day every 5 days of observation. We therefore preferred to clearly show our data shortages. For the revised manuscript, we included also measurements from MIPAS middle atmosphere mode, which extend down to 18 km altitude. MIPAS upper atmosphere mode and noctilucent clouds mode do not cover the lower stratosphere and were therefore not considered for this analysis. See a detailed description of MIPAS observation modes at http://www.atm.ox.ac.uk/group/mipas/L1B/. This implies that part of the data gaps have now been filled at least down to theta around 450 K. Also, in the original manuscript data gaps appeared larger than expected because of plotting shortages at the edge of data gap. We further investigated the behavior of the vortex through ECMWF sPV data and found that none of those 1 day data gaps present conditions which are different from adjacent days. In order to improve the readability of the timeseries, we therefore decided to allow interpolation through 1-day data gaps and limited the use of white stripes to periods with data gaps larger than 1 day. Furthermore, in the revised Figures 1 and 2, white vertical stripes now cover the exact time period of the remaining data gaps. 1-day data gaps that are not shown are on 2010: 5, 15 and 25 December; and 2011: 4, 14, 24 January, 13, 23 February, 5, 15, 25 March. We added a statement on the dates of 1-day data gaps to the revised manuscript, Sect. 2.2 on MIPAS2D data.

5. Sect. 3.3.4., page 33208: "… 100% depletion of O3 in a fraction of the vortex in late March and early April." Have the authors of this paper checked this statement carefully? Sinnhuber et al. presented on 31 March 2011 a vortex minimum value of ozone of 0,3 ppmv (at about 18km altitude).

The results of zero ozone discussed in this section were checked carefully and were obtained with different configurations of the 2D retrieval code (i.e. were not due to a random numerical event). Sinnhuber et al. make use of a 1D retrieval code adopting a Tikhonov regularization. There are therefore a few substantial differences between the two codes, among which for example different microwindows and the regularization in the 1D which in principle may affect the retrieval when trying to obtain zero values of ozone in a very limited vertical region as is the case with some of the 2011 Arctic ozone profiles. Our 2D code has no vertical smoothing a part from the indirect effect of a coarser vertical grid and the use of the Marquardt damping factor. The code itself is known to lead to profiles which tend to oscillate more as compared to profiles which are smoothed vertically. However, in the particular case of these zero-ozone measurements, no oscillations are seen in the profiles that reach zero (o nearby values) around 18 km altitude. Moreover, as discussed in the manuscript, about 10% of the vortex measurements are consistent with a zero value within the associated errors. Considering that the values refer to individual retrieved data, random errors on ozone are of the order of 0.15 ppmv, so that both the 0 and 0.3 values obtained by the two studies may be considered consistent within the given errors. Which of the two codes can handle better such a strong vertical gradient will need to be investigated elsewhere. A detailed study joining the two dataset is in fact out of the scope of this manuscript, but would certainly be of benefit for the community. Activities in this sense are carried out by a joint validation of MIPAS products. In the conclusions of our revised manuscript, we commented on the newly published Sinnhuber et al. [2011] work together with Manney et al. [2011] work, and introduced a comment on this difference in minimum O3 values.

6. Sect. 4.1, page 33210: "PSCs reaching altitudes above 30 km." It is recommended to intercompare the MIPAS data with the results of the CALIOP-Lidar Experiment in order to confirm this new finding.

Indeed the PSC detection at high altitude was compared to CALIOP measurements, the latter pointing to an event on 4 January which clearly extended above 30 km altitude (the available CALIOP map of a cloud on that day was shown to be abruptly cut at 30 km altitude whereas its shape was undoubtedly extending above that altitude). This motivated the inclusion of our unexpected finding in the ACPD manuscript, adopting a slightly more relaxed criteria for the detection method to allow full discussion with the community. Following the suggestions of the reviewers, we adopted more stringent criteria on our PSC detections. As a consequence, we maintained two cases of PSC detected by MIPAS at 30.2 and 30.5 km altitude (centre of MIPAS field of view), consistently with the high altitude PSC observed by CALIOP. Please see full discussion in the replies to referee #1 and #3.

C) Technical comments

page 33193, line 11: "… chlorine molecules are converted into…"

ok

page 33194, line 4: "… ranged between 25% and 35% in…"

ok

page 33197, line 4 from bottom: Replace “clean” by “clear” !
ok
page 33204: Replace “continue” by “continuous” !
ok
page 33206, line 7: Replace “… actual denitrification …” by “… denitrification must have occurred previously.”
ok

Various figures have to be improved because several curves or symbols respectively cannot be identified unambiguously:
Figure 3/4: outer and inner edge of vortex
Inner and outer vortex edges were plotted with lines of different thickness to ease their identification by the reader.

Figure 5: vortex edges (black lines)
Inner and outer vortex edges were plotted with lines of different thickness consistently with Figure 3 and 4.

Figure 7: gray circles
See reply below.
Figure 8: gray lines
Both figures - We do understand that using different colors/symbols for different years would allow the identification of a particular year. However, when attempting to do this, the plots become rather unreadable and the 2011 data less visible. We prefer to show how 2011 compares to the spread of previous years data, using a gray circles/lines for all years rather than the envelope, so as to allow understanding where most years tend to fall. Please note this is the same approach followed by Manney et al. 2011, Nature, in their Figure 3 of the Supplementary Material, where only a few meaningful years were highlighted with colors. In our manuscript, we highlighted 2011 in color and the vertical profiles for 2007 with dashed lines. Following the reviewer’s comment, as well as reviewer 2’s suggestion, in each panel of Figure 7 we introduced the average of previous years as a thick gray line. This will further aid to prompt compare 2011 conditions to previous years.