Response to Anonymous Referee #1: 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.

1) General comments:

The manuscript by Arnone et al. presents a detailed analysis of the 2010-2011 Arctic winter observation of the MIPAS instrument. A unique pole-covering dataset of trace gas measurements and polar stratospheric clouds observations allows a comprehensive analysis of this unique winter in respect of ozone depletion and evolution of the polar vortex in general. The paper is well written, some technical changes will improve the quality of the manuscript (see comments below). From my point of view there is only one but really critical point to address in more depth before publication is possible in ACP. The detection of PSCs above 30 km altitude seems unrealistic in respect to the current state of research. The detection method and applied thresholds are not validated at such altitudes. Consequently the presented results are not reliable. However, MIPAS is very sensitive for the detection of optical thin clouds and it is definitively worth but also essential to investigate these indications for high altitude PSC events in more depth. The authors have to convince the reviewers and the scientific community that these events are not artefacts of the data analysis.

We are grateful to the reviewer for the comments to our manuscript and in particular for the care in addressing the results we obtained with the PSC analysis, which is one key result of our study. Indeed we preferred to bring to the scientific community these high altitude MIPAS PSC detections which were unexpected and extremely valuable for the understanding of the Arctic stratosphere, although at the expense of adopting some more relaxed assumptions on the PSC detection technique. Some of these MIPAS detections of high altitude PSCs had a very robust PSC signature, others a much weaker one. The motivation for carrying all of these measurements up to the discussion phase in ACPD came from coincident measurements from CALIOP/CALIPSO indicating existence of PSCs above 30 km altitude in the Arctic during 4 January 2011. In order to avoid introduction of false detections, we revised all PSC detections with a much more conservative approach and further looked for evidence of PSC signatures directly in MIPAS spectra. As a result, we confirm PSCs were detected by MIPAS above 30 km altitude in two measurements on 3 January 2011 (with field of view centered at 30.5 and 30.2 km altitude for the two cases), consistently with a similar detection by CALIOP/CALIPSO on 4 January, whereas other higher altitude measurements were not confirmed by the more stringent investigation. Detailed answers are given here below.

2) Detailed comments:

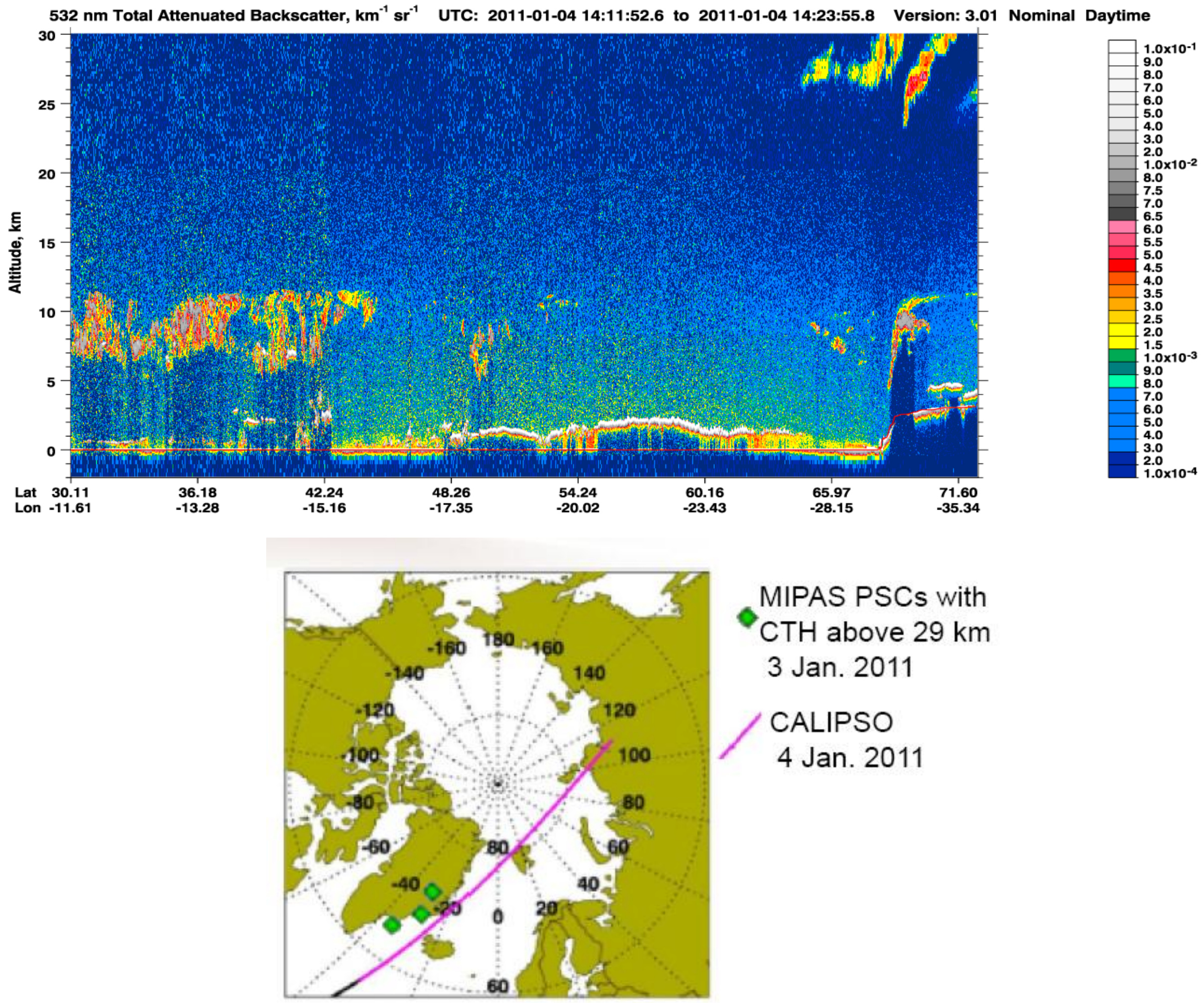
PSC observation above 30 km:

The selection of your threshold values and upper altitude are not correctly cited in the manuscript (section 2.3). Spang et al. (2005) used CI=4 between 14 and 30 km, below and above they choose CI=1.8. The more ‘relaxed’ definition by Arnone et al. of CI=4.5 above 30km may produce false detections. This problem becomes already obvious in Fig. 2 of Spang et al. 2005, where above _28-30km the CI profile starts to become noisy and CI-values close to 4.5 are possible for observation in winter 2002/3. The noise is caused by the very low stratospheric temperatures in polar vortex and corresponding low signals in the measured spectra. More recent analyses by Spang et al. (2011) take this problem into account and end up with significant smaller threshold values above 30km (Fig. 3). The authors should address the described difficulties in their analysis by checking the individual CI-profiles and the corresponding spectra. Latter should show for cloudy cases a significant radiance offset in respect to the baseline. I expect that none of the detected high PSC events will withstand such a detailed analysis. If the results show still a significant amount of high altitude PSCs, then it would be a remarkable result and should addressed in more detail in the manuscript (e.g. comparison with CALIPSO coincidences).

The motivation for maintaining MIPAS measurements with CI=4.5 above 30 km up to the discussion phase in ACPD came from coincident measurements from CALIOP/CALIPSO indicating existence of PSCs above 30 km altitude in the Arctic during 4 January 2011 (see Figure 1). In order to avoid introduction of false detections, we revised all PSC detections with a much more conservative approach as suggested by the reviewer and further looked for evidence of PSC signatures directly in MIPAS spectra. In particular, we adopted a CI=4.5 below 30 km altitude, and a CI=1.8 above that altitude. The 1.8 threshold is consistent with Spang et al. 2005, whereas the 4.5 threshold is consistent with Spang et al. 2003 and Hoepfner et al. 2006, 2009. Moreover, the analysis by Spang et al. 2011 ACPD (as suggested by the reviewer), pointed to a much higher CI index below about 28 km altitude in the polar region so as to confirm our adopted thresholds as conservative assumption. We prefer to maintain published values rather than introducing the

Ci profiles reported by Spang et al. 2011 for the following reasons: 1. the manuscript is going through a discussion and revision phase and their new CI profiles may change, therefore affecting the robustness of our analysis; 2. The CI profiles reported by Spang et al. 2011 have an abrupt change passing from the polar region (65 to 90 degree latitude) to the mid-latitude band (20-65 degree latitude). Since Arctic 2011 PSCs often extended down to 60 degree North, this suggests we should use interpolated values for the region around 60-70 degree, although we are not confident this is the right approach. Nevertheless, interpolated values of the suggested profiles would still fall around the CI=4.5 threshold we adopt; 3. A test performed applying Spang et al. 2011 criteria to our data almost doubled the number of PSC detections (> 5000 as compared to about 2900 obtained with the previously published methods), therefore extending the PSC detection to much thinner ones. Unfortunately, this new method led to introduce a very large number of PSCs at low altitude also in December (well below the altitude of existence of the T_NAT regions shown in Figure 1b of our manuscript). We further investigated the occurrence of these low altitude PSCs in comparison with CALIPSO data and found no correspondence at all, therefore pointing to an increased sensitivity of MIPAS detection through this novel method, or the introduction of false detections. The investigation on the quality of this new method is out of the scope of our manuscript. Because of these reasons we feel we should maintain our original approach based on previously published literature, with the refinement of a more conservative CI index above 30 km altitude.

As a result of this refinement of the analysis, we confirm PSCs were detected by MIPAS above 30 km altitude in two occasions on 3 January 2011 (with field of view centered at 30.5 and 30.2 km altitude for two cases), consistently with a similar indication by CALIOP/CALIPSO, whereas other higher altitude measurements were not confirmed by the more stringent investigation. These two MIPAS PSC detections were confirmed by direct eye inspection of their spectra, clearly showing a broad-band enhancement of the recorded intensity. CALIPSO data were available through the CALIPSO Browse Image at the NASA (http://www-calipso.larc.nasa.gov/products/lidar/browse_images/show_calendar.php): for the first day of January 2011 a persisting PSC layer is detected at altitude levels well above 25 km (CTH from approximately 27 to 29 km). In particular on 4 January 2012 in some granules (e. g. from 04:30 UTC to 04:44 UTC, or from 14:11 UTC to 14:23 UTC in Figure_reply 1, top panel) CALIPSO detected a PSC layer with CTH that was clearly extending higher than 30 km (CALIOP upper boundary). This measurements by CALIPSO are compatible in geolocation with those found by our analysis of MIPAS spectra for 3 January 2012, being both very close to Greenland. Given the stringent analysis we performed, and based on previous literature on MIPAS PSC detection, we believe that the MIPAS PSC measurements we report at 30.2 and 30.5 km altitude are robust and that no detailed comparison to CALIPSO data should be needed in the text. We further highlighted this finding in the abstract of our manuscript and added further details in Sect. 3.2.



Figure_reply 1: TOP - 532 nm Total attenuated backscatter from CALIOP for the granule at 14:11 UTC. Very high PSC clouds can be seen on the image upper right corner.
(from http://www-calipso.larc.nasa.gov/products/lidar/browse_images/show_calendar.php)
BOTTOM – CALIPSO orbit corresponding to top panel and MIPAS PSC detections with cloud top above 29 km altitude.

Title:

I go along with reviewer #2 and suggest a change of the slightly misleading term 'total depletion of ozone' in the title. Please address the limitation on specific atmospheric layer.

The title intended to point to the peculiarity of the zero-ozone measurements 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"

Introduction:

Please explain the term 'Volume of PSC' in more detail, which is not really the volume of PSC formed in the polar vortex but a proxy of potential PSC formation by a simple temperature threshold.

Reference to the "Volume of PSC" was revised as "the volume of air having temperature below the threshold for PSC formation".

Section 2:

The orbit parameters of Envisat are changed since end of 2010. This should for example produce a drift in orbit node (equator crossing time, etc.). Please clarify.

Indeed Envisat orbital parameters have slightly changed since the end of 2010 to optimize the fuel consumption and extend the satellite operational lifetime. This orbital change introduced minor changes also in the equator crossing time, which is now cycling within +_5 minutes around 10:00 pm. These changes have no impact on the retrieved products.

Section 2.3 PSC detection and composition:

- Cloud Index technique for PSC detection is first published by Spang et al. (2001).

The reference to the cloud index technique was revised to “Spang, R., M. Riese, and D. Offermann (2001), CRISTA-2 observations of the South Polar Vortex in winter 1997: A new dataset for polar process studies, Geophys. Res. Lett., 28(16), 3159–3162, doi:10.1029/2000GL012374.”

- Please address that the classification by Höpfner et al. is based on modelled cloud spectra.

The reference was clarified as suggested.

Section 2.4:

Is it not the sPV gradient which defines the vortex edge? Have you checked the vertical distribution of sPV?

The vortex edge can be identified by the region of strongest PV gradient (which coincides with the core of the polar night jet). However, several previous studies (see e.g. Manney et al 2006 as cited in the same Section of our manuscript, or the same Manney et al. 2011) have shown the vortex edge can be closely traced by an absolute value of sPV, which is introduced to have a similar range of values over isentropes throughout the stratosphere. The gradient of PV is better suited to study the strength of the vortex barrier. The sPV threshold we selected was indeed obtained through inspection of its vertical distribution in the stratosphere throughout the winter. Given the large number of measurements included in the vortex averages (typically 80), the results are not largely affected by a change in the vortex edge definition itself, but rather by inclusion of non-vortex air in the vortex averages. We avoided the latter case by adopting a slightly more conservative value of the sPV as compared to other studies (e.g. Manney et al. 2006, 2011).

Section 3:

I have some concerns about how the data gaps by PSC are handled in your analysis / 2-D approach with fixed latitude-altitude grid. You should note somewhere close to the introduction of Figure 3 and 4, if and where PSC may hamper the trace gas observations in the vortex. E.g. Fig. 5 suggests extensive PSC coverage on March 8, but Fig. 4 shows no data gaps in the trace gases. Is this an effect be the different altitudes?

Cloud contaminated measurements are rejected from the 2D analysis when the CI is smaller than 4. This slightly lower limit as compared to that used in the PSC detection (CI<4.5) already allows for a fraction of measurements to be used both as PSC detection and for the retrieval of other atmospheric parameters. In most cases where PSCs are optically thicker however, cloud-contaminated data are rejected from the retrieval analysis. In these cases either we do not retrieve a profile, and therefore the vortex average is then based on a smaller number of profiles, or the 2D retrieval gathers information from the following MIPAS measurements (or measurements at higher altitude) whose lines of sight cross a nearby air mass without PSCs. Both situations are kept under control during the data analysis for this study, by requiring a minimum number of retrieved data points for each bin of the vortex averages, and by applying a minimum threshold to the information gain of each retrieved data point accepted for the analysis. Indeed regions with the largest coverage of optically thick PSCs will lead to vortex averages being biased by contribution of the vortex outside the PSC region. During the revision, we have increased the minimum number of data points for each averaging bin to 10. The vast majority of averaging bins have however around 80 data points, showing that the impact of PSCs is rather limited. Because of this change, in Figure 1 and 2, some of the lowest altitudes have now a slightly smaller coverage as compared to the ACPD manuscript, and we rejected one day with too little data.

The threshold for the minimum information gain for accepting a retrieved data point was kept to 0.5 (although inspection of the distribution of the information gain shows very few cases have this low value). If this value is reached at a certain grid point even when one of measurement is rejected because of PSC contamination, enough information was gathered from the other nearby measurements. In case of no information available in the nearby measurements, the information gain drops to smaller values and the retrieved data point is not accepted.

Section 3.1:

- The discussion on N2O and CH4 is slightly confusing, please specify e.g. ‘impact of chemistry on CH4’

We revised the sentence mentioning that the agreement between N2O and CH4 passive tracers is generally good, although some differences between the two tracers in early spring will need to be further investigated.

- Can you define the date of the final warming, if yes, then please add.

The final warming was evaluated as 19 April in the NCEP-2 reanalysis by M. M. HurwitzP. A. Newman, and C. I. Garfinkel, Atmos. Chem. Phys., 11, 11447.11453, 2011 www.atmos-chem-phys.net/11/11447/2011/
We added a comment to our manuscript.

Section 3.2:

- I can not follow the arguments of 'clear STS cases' and/or signatures. The classification is not able to discriminate 'clear' STS events, only STS/mixed clouds, which can include NAT or are even dominated by NAT particles with r>3microns.

We agree the adoption of the term clear in "clear STS cases" is not consistent with the adopted classification. We implied that the in those cases the PSCs fell well below the line separating the NAT and the STS/Mix regions in the diagram, but indeed even in those cases NAT particles of very large radii would overlap with the STS signature. We have removed "clear" from the sentence.

- What is the reference or argument for the 'at least 40%' NAT particle clouds?

This is based on Hoepfner 2006 and 2009 as stated in Sect. 2.3 describing the adopted method, and recalled within the paragraph. The level of 30-40% of NAT particles within the sounded PSC was found by Hoepfner et al. 2009 (their page 4, second column, line 3) as the minimum density needed to lead to PSCs falling within the NAT region of their diagram. Hoepfner et al. 2006 also showed that only NAT particles with radii smaller than 3 µm would fall in the NAT region. Following the reviewer's comment we recall Hoepfner 2006 and 2009 within the paragraph of the quoted sentence.

Section 5:

The last paragraph of the conclusion is confusing me. I don't get the point you like to address. What do you mean with 'scattered low values'?

We refined the last paragraph pointing out that MIPAS2D are suitable for studying regions where filaments or scattered values (inhomogeneous conditions) are present, as in the case of the Arctic vortex.

3) Technical Comments:

p33194: 'Eventually, Arctic . . ..' sounds quite unspecific.

Following also Reviewer #3 we changed "Eventually" to "Recently" and avoid further discussion of the 2010-2011 since it is the topic of our analysis.

p33195: change '. . ., unapodised' to '. . . (unapodised)'

OK.

p33197, l25: 'optically' thick clouds and 'estimated' instead of derived

OK "optically thick clouds". Following also our answer to the reviewer's question on Section 3 (on "*how the data gaps by PSC are handled in your analysis / 2-D approach*"), when data are retrieved and accepted, they are indeed derived (as adopted in our text) rather than "estimated" as suggested by the reviewer. We therefore maintain the term derive ed.

p33198, l27; reference for the definition of the NAT index should be added (Spang and Remedios, GRL, 2002).

The suggested reference was added to the revised manuscript.

p33200: 'Figs. 1 and 2' may change to Fig. 1 and 2

The abbreviation "Figs." is used throughout the manuscript in accordance to ACP conventions.

p33200, l15: The formula for T_NAT is based on lab measurements, please delete the term 'empirical'.

OK.

p33202 l7: I suggest '. . . based on the method described by Höpfner et al (2006).'

This should refer to p33203 l7. We adopted the suggestion in the revised text.

P33204 l17: the term 'spectral signatures' is misleading for the data presented in Fig. 5

We changed spectral signatures to spectral characteristics.

P33204 l24: . . . 'optically' thick

OK.

P33205 l12: you should give a reference for the statement of large NAT particles.

Reference to Hoepfner et al. 2006 and 2009 was added within the paragraph of the quoted sentence.

4) Figures:

Fig. 1: Some numbers for white contours of N2O would be helpful

We refined the numbers on the white contours of the N2O plots.

5) References:

Spang, R., Arndt, K., Dudhia, A., Höpfner, M., Hoffmann, L., Hurley, J., Grainger, R. G., Griessbach, S., Poulsen, C., Remedios, J. J., Riese, M., Sembhi, H., Siddans, R., Waterfall, A., and Zehner, C.: Fast cloud parameter retrievals of MIPAS/Envisat, Atmos. Chem. Phys. Discuss., 11, 33013-33094, doi:10.5194/acpd-11-33013-2011, 2011.

Spang, R. and Remedios, J.: Observations of a distinctive infra-red spectral feature in the atmospheric spectra of polar stratospheric clouds measured by the CRISTA instrument, Geophys. Res. Lett., 30, 1875, doi:10.1029/2003GL017231, 2003.

We included references to Spang et al. 2001, 2002 and 2003. We prefer not to include reference to Spang et al. 2011 ACPD since the manuscript is under discussion and for the reasons discussed above (mainly because the application of their method led to a large number of PSCs being detected in regions where CALIPSO observations had no detections. This would require a validation against CALIPSO and ground-based data which is out of the scope of our manuscript).