Response to Anonymous Referee #3: 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.

Overall comment: This manuscript presents results for the Arctic winter of 2010/2011 based on 2D tomographic retrievals of ENVISAT/MIPAS data and ECMWF analyses. The MIPAS2D retrievals seem to be of high quality, and what they show about the extraordinary stratospheric conditions of the 2010/2011 winter is of general interest. However, in my opinion the manuscript contains numerous inaccuracies and flaws in either the analysis or the exposition of it (or both), and it will require substantial revision before it is suitable for publication in ACP. Both major substantive issues and minor wording comments are enumerated below.
We are grateful to the reviewer for the many comments and suggestions which have contributed to improve our study. We went carefully through all the points that were raised and applied to our manuscript most of the changes that were suggested. On some points, the interaction with the reviewer (together with the other reviewers) have greatly helped in making our key results more robust. The quality of our results was challenged requiring to improve their description or the adoption of a more conservative approach. This is particularly the case of the PSCs analysis, for which we adopted a more robust detection threshold in order to avoid false detections: some very high altitude PSCs could not withstand the more stringent criteria and were discarded, while our main affirmation of detection of PSCs above 30 km altitude was confirmed (although by two measurements only).

On a few issues we maintained our point of view and disclosed some further pieces of evidence to support our analysis in this document, as we believe these are not appropriate for inclusion in the manuscript itself. This is for example the case with the adoption of MIPAS2D average temperatures, self-consistently with all other targets, rather than ECMWF ones. For some other issues we rely on relevant literature and refer to specific examples of similar studies. Overall, we believe our main results were confirmed and made more robust by the review process and by the careful work of the reviewer. Detailed answers to the reviewer's comments are given here below.

Substantive comments:
– p33193: L12-14: "... chlorine is converted into active radicals such as Cl and ClO, which destroy most of the vortex ozone at 14-20km altitude (Solomon et al., 1986; Molina et al., 1987)". Solomon et al. and Molina et al. are not the best references for the second part of the sentence, for which a recent WMO report would be more appropriate.
Reference to the latest WMO report was added.

– p33193, L17-18: "Denitrification of the Antarctic stratosphere through sedimentation of HNO3 prevents the reactive NOx to promptly recapture the available chlorine into reservoirs". This sentence needs to be more carefully worded. Denitrification only prevents chlorine deactivation through the formation of ClONO2; it has no impact on the production of HCl, which is the primary deactivation pathway in the Antarctic.
The sentence was updated specifying that denitrification prevents chlorine deactivation through the ClONO2 channel.

– p33193, L24-25: "The total ozone depletion in the Arctic winter was found to be linearly dependent on the volume of PSCs integrated over the winter (Rex et al., 2006)...". This relationship has also been confirmed for the Antarctic by Tilmes et al. [GRL, 2006].
Tilmes et al. [GRL, 2006] indeed applied calculations of the volume of PSCs (V_PSC) and of the PSC formation potential (PFP) to both the Arctic and the Antarctic. However, they concluded that "saturation" of the chemical loss of ozone is reached in the Antarctic so that no significant change as a function of PFP (or V_PSC) is observed. The linearity of the relationship does not hold anymore (at least at potential temperature between 350 and 550 K) once the saturation is reached. We therefore included reference to Tilmes et al. in terms of minimum V_PSC for complete ozone destruction been exceeded in the Antarctic every year.

Please note that the notion of "volume of PSCs" was updated in the revised text as "volume of air having temperature below PSC formation" following a suggestion by Reviewer #1.

– p33194, L12: "Eventually, Arctic ozone hole conditions were reported for the 2010-2011 Arctic winter (Manney et al., 2011)". Using the word "Eventually" here makes it sound as though it was inevitable that ozone hole conditions were reached in the Arctic. I don't think that's true. It might be better to say something along the lines of "Recently, Arctic ozone hole conditions were reported for the first time for the 2010-2011 Arctic winter (Manney et al., 2011)".
Yes, the word "eventually" may be misleading if read in this way. We replaced it with "recently".

– p33196, section 2.2: The MIPAS2D retrievals at the core of this work are briefly described in this section, but no information is provided about their quality, that is, accuracy, precision, and resolution (the retrieval vertical grid is specified, but that does not necessarily reflect the true vertical resolution of the data). It is stated that the MIPAS2D database is "thoroughly described in Dinelli et al. (2010)", but readers should not have to look up another paper to obtain information that is critical for evaluating the reliability of the results presented here.
A brief summary of the accuracy and precision of MIPAS2D data was added to the section.

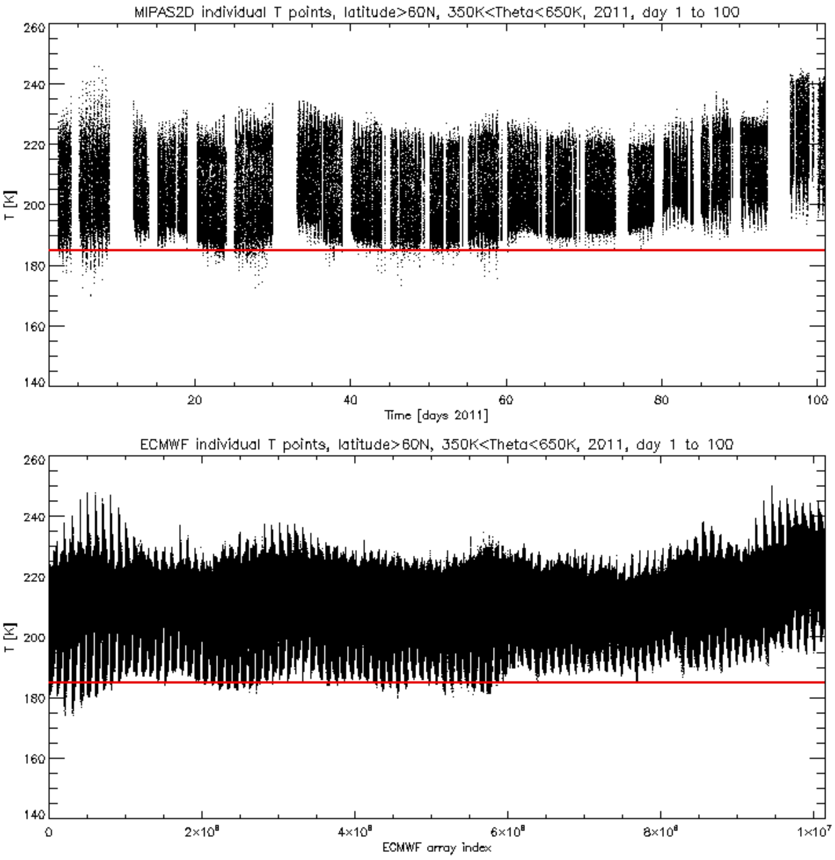
– p33201, L9-10: "... regions of temperature below TNAT lasting almost continuously until late March". In fact, in at least some meteorological analyses, minimum temperatures were below TNAT continuously from early December until the beginning of April. It is only the MIPAS data that are discontinuous in this interval, not the low temperatures.
Indeed also in our data minimum temperatures were below TNAT continuously in some regions until the beginning of April, so that "almost" could be removed from the sentence.

– p33201, L12-16: "Temperature minima dropped persistently below 185K ... The presence of these persistent cold regions is reproduced in the vortex average temperature ...". First, I do not think that vortex average temperature is a particularly meaningful quantity. I think it would be much more informative to show minimum temperature in Fig. 1, rather than the vortex mean value. Second, it is rare for minimum temperatures to drop below 185 K in the Arctic. Although 2011 was a cold winter, temperatures were not extraordinarily low (they were just moderately low for an extraordinarily long time, as shown in Manney et al. [2011]). Thus I am not convinced that such low temperatures were reached at all in 2011, and certainly not persistently for days at a time, as is asserted here. Had temperatures really been that low for that long, many more ice PSCs would have formed and a greater degree of dehydration would have been observed. This leads me to suspect the quality of the MIPAS temperature data (as noted above, no information on data quality has been provided in the manuscript for most of the MIPAS data products). How reliable are the MIPAS temperatures? How well do they compare with, for example, ECMWF? My guess is that the ECMWF temperatures are more suitable for polar processing studies than the MIPAS retrievals. Since ECMWF temperatures are used elsewhere in this paper, why not use them here too instead of the MIPAS data?
There is an extended literature on the polar stratosphere making use of either vortex average temperature, average temperature of the cold pool region, or minimum temperature. We agree with the reviewer that the vortex average temperature on its own does not give a clear picture of the vortex conditions. This is the reason why in panel (b) of Fig. 1 we also report the average temperature of the coldest region of the vortex (the T_NAT region, or cold pool). We believe the use of minimum temperature may not be preferable because of the following reasons: Firstly, the minimum temperature may be reached in an extremely small region of the vortex, making it not representative of the overall conditions of the cold pool. This is in fact the case of the 2011 Arctic vortex, where temperature dropped below 185 K for several days in January and February, although in limited regions. Secondly, individual temperature data points are affected by measurement random errors which can be greatly reduced adopting averages.

Regarding the quality of MIPAS2D temperature data, there are no known biases in the lower stratosphere in comparison to other datasets. An ongoing effort to validate MIPAS2d data has shown good agreement with in situ measurements (see also Tomasi et al. 2010, full reference in the manuscript). Furthermore, the adopted 2D retrieval allows the full modeling of horizontal temperature gradients which were shown to be essential to avoid errors as high as several K (Kiefer et al. 2010, full reference in the manuscript). Although one could use ECMWF temperatures also for describing the vortex conditions throughout the winter (we indeed adopt them for assigning the local temperature to individual PSCs since we have no temperature data in their coincidence), MIPAS2D temperatures are extracted from the very same measurements used for the chemistry data, so that self-consistency in space and time is guarantee (ECMWF data would e.g. need to be interpolated in time to obtain data at 10 am and 10 pm local time resembling MIPAS measurements). Moreover, we adopt a multi-target retrieval approach by which pressure, temperature, ozone and water vapor are retrieved simultaneously from the spectra, so that the resulting temperature is the best estimate to accompany the retrieved values of the chemical species. Clearly adopting MIPAS2D we missed some of the temperature data in coincidence with PSCs. On the other hand, since ECMWF is based on its model core, one may question whether it is more trustable for representing regional low temperature in regions where the assimilated observational data does not lead to a good coverage.

We believe this manuscript should not include a comparison of MIPAS2D data with ECMWF or other dataset. We therefore report only within this answer (Figure_reply 1) a comparison of MIPAS2D temperature data points with ECMWF data points over the first 100 days of January, North of 60 degree latitude and in the lower stratosphere at

350K<Theta<650K. MIPAS2D data come from successive orbits and they are reported as a function of measurement time. ECMWF data are reported in terms of the array index which runs through all longitudes, latitudes, theta levels and time (each spike in the figure corresponds to one day), over the same time period. For convenience, a red line in each panel indicates the threshold for temperature = 185 K commented on above. The overall agreement is very good, also including the low temperature data points and the days when these are reached. The fraction of sample data with temperature below 185 K is 0.37% in MIPAS2D and 0.29% in ECMWF. ECMWF temperatures dropped to or below 185K in about 40 days during the Januar and February, especially in the periods 2-7 January and 20-28 January, indeed persistently for several consecutive days, consistently with what discussed in the manuscript. We therefore consider the sentence “Temperature minima dropped persistently below 185 K at Theta from 500 to 750 K in 2-7 January, and at Theta from 400 to 650 K in 20-28 January and sporadically throughout February” consistent with our finding.



Figure_reply 1 - Top: MIPAS2D temperature data points during the first 100 days of 2011, latitude > 60 degree North, 350K<Theta<650K as a function of measurement time. Bottom: ECMWF temperature data points during the first 100 days of 2011, latitude > 60 degree North, 350K<Theta<650K, as a function of an array index running through all longitudes, latitudes, theta levels and time.

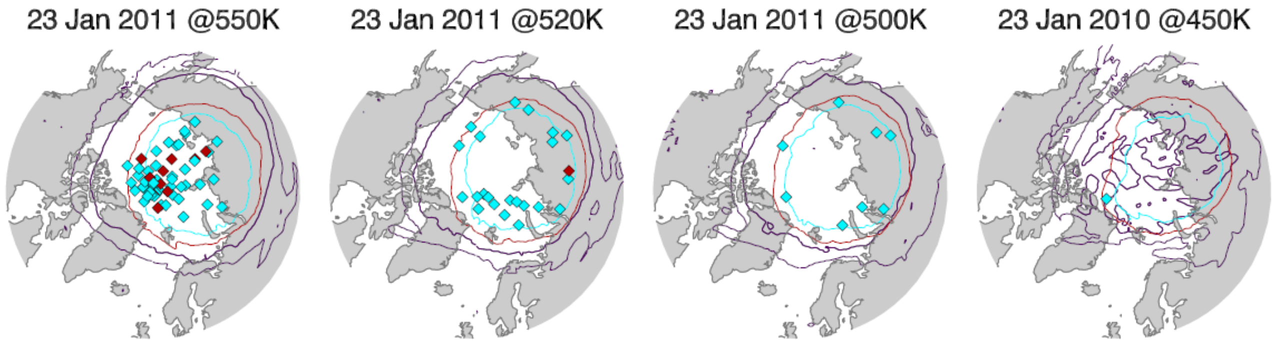
– p33202, L20-21: "Large concurrent increases in temperature and the N2O tracer showed the Arctic return to typical conditions." It’s not clear when this occurred. The previous sentence referred to late April, so perhaps this one does as well, but that should be clarified. The reader cannot judge either of these sentences because the plots in Figs. 1 to 4 only extend to late March or mid-April.
We added specific mention to April in the revised manuscript.

– p33202, L23-25: "A total of 2920 MIPAS scans detected PSCs over the period 1 December 2010 to 18 March 2011, corresponding to 82% of days with PSCs out of 92 days of observation". Earlier (Section 2.3) it was stated that MIPAS identifies about 70% of the PSCs detected by CALIPSO in the Arctic. So clearly some PSCs are missed. Are there false positive detections as well? These statistics have implications for results discussed later in the manuscript.
As stated in Section 2.3, previous studies by Hoepfner et al. 2006 showed that MIPAS detection limit of PSCs with volume density of about 0.2 μm^3 cm^{-3}. Optically thinner clouds will be missed. A part for the few cases of weak signatures of high PSCs we kept in the ACPD for further discussion, the other detections are robust and based on clear

cloud signatures in the recorded spectra. So no false detections are expected. Please also see detailed answers to reviewer #1 on PSC detection and classification. The comparison to CALIPSO made by Hoefpner et al. 2009 indeed showed about 70% of PSCs detected by CALIPSO were identified by MIPAS, although other factors (e.g. correct coincidence geolocation) have a weight on this number. Also to consider is the much larger fraction of PSCs that MIPAS can cross since it covers up to the North Pole, whereas the CALIOP nadir lidar on CALIPSO can cover only up to about 80 degree North. A much larger number of detections (> 5000) may be possible in our MIPAS dataset with an improved method suggested by reviewer #1, i.e. adopting new thresholds presented by Spang et al. 2011 ACPD. However, we pointed out that the new method led us to identifying a large number of PSCs in days where no CALIPSO detection were present. Whether this implies a larger sensitivity of MIPAS limb measurements to very optically thin (and possibly laterally extended) clouds it is to be further investigated by dedicated studies. So indeed we can be robust on the detection of PSCs which are not too optically thin (or for example fill only part of MIPAS field of view), but a fraction will always be missed. What it is less likely is having days when MIPAS misses all PSCs, unless of course only very optically thin clouds or few sparse PSCs were present: although this can happen, it has also a smaller effect on the chemistry so that the overall analysis is relatively unaffected.
Please note that with the adoption of the more conservative thresholds for cloud detection the number of PSCs detected by MIPAS decreased from 2920 to 2876 in the nominal observation mode (the only one adopted for the original manuscript). In the revised manuscript, we included also measurements from the middle atmosphere observation mode, which carry an extra 244 detections. The new total number of PSCs detected by MIPAS in our analysis is therefore 3120. The number of days of MIPAS observations between 1 December 2010 and 18 March 2011 (period between the first and last detected PSCs, i.e. the PSC season) increased from 92 to 97 (some of the days covered by the middle atmosphere mode were already partially covered by one or two orbits of the nominal observation mode). The total number of days with PSCs increased from 73 to 76 (as a consequence of losing one day with one detection now discarded by the new more conservative CI threshold, and introducing 4 more days with PSCs with the middle atmosphere mode). This corresponds to a fraction of 78% of days with PSCs during the PSC season.

– p33203, L2-5: "... close to the cold core of the vortex for most of the winter, tracing the occasional distortion of its shape". I am confused by this wording. What is meant by "tracing"? Perhaps "following" would be a better word. Is it the whole vortex or just the cold core that gets distorted?
We revised the sentence as "…, following the occasional distortion of the vortex shape."

"The only evident deviation was observed between 20 and 29 January, when detected PSCs shifted from the cold core of the vortex to form a ring surrounding it". I assume that this ring of PSCs surrounds the cold core of the vortex, and not the vortex itself (i.e., it does not lie outside of the vortex). Perhaps the implications of the PSC formation region shifting from the coldest part of the vortex core to warmer areas could be discussed more. Did this change in the location of PSC formation occur for both the upper and the lower altitude PSC regions?
We further investigated the occurrence of this ring of PSCs. We found that, considering the whole vertical span of the measurements, the detected PSCs followed an almost bell-shaped distribution: higher layers (550-600 K) have detected PSCs in the cold core of the vortex, lower layers (500-520 K) in a ring surrounding it, a ring which gets progressively larger towards lower altitude. An example of this configuration on the 23 January 2011 is given in Figure_reply 2. Since our analysis of MIPAS PSCs considers only the cloud top (as discussed in the methodology section), PSC at low altitude below higher altitude ones will not be detected. It is therefore very likely that a selection bias is introduced for PSCs in the inner cold core of the vortex at low altitude during this particularly cold period. We further checked this possibility with CALIPSO data and found that indeed in some of CALIPSO orbits crossing the centre of the vortex, PSCs are detected also in the inner part of the vortex at low altitude. The comparison is however not straightforward as CALIPSO data do not cover the bulk of PSCs detected by MIPAS which lie North of 80 degree latitude. An investigation of this particular situation should be carried out in detail in a separate study. We therefore mention these conditions in the manuscript but remove the quoted sentence about the ring.



Figure_reply 2 – MIPAS detected PSCs (diamonds) on 23 January 2011 at theta= 550K, 520K, 500K and 450K (left to right). PSCs are classified as NAT (red) or STS/Mix (cyan). Contours of the regions for the expected formation of NAT or STS are shown with a bold line (respectively in red and cyan – see further details in the manuscript). Black lines indicate the vortex edges.

– p332043, L25-27: "Although the majority of PSCs were vertically distributed where the average temperature was consistently around the threshold for STS formation (Fig. 1b), temperatures associated to the PSCs of this period were the highest of the 4 periods". Again, because PSCs are highly localized, the vortex average temperature is almost totally irrelevant. In fact, although one can tell from Fig. 5 that period (i) was warmest overall, Fig. 1a shows that in a vortex-average sense, temperatures in the first half of January were actually higher than they were in the last half of December. This underscores the point that vortex mean temperatures are much less useful in this context than minimum temperatures.

See discussion above regarding the use of cold pool average temperature instead of minimum temperature. Indeed we referred to the average temperature of the T_NAT region. We revised the overall description of PSCs and temperatures taking care of referring to the temperature of the T_NAT region and not to the vortex averaged temperature. Comparison of the vortex mean temperature and that of the T_NAT region allows to understand how representative for the overall vortex were the conditions in the cold pool.

– p33204, L2 and L16: For period (ii) it is stated in L2 that "a very few cases" were classified as ice PSCs. I see only one point clearly in the "ice" category in Fig. 5b. In addition, in L16 the statement is made: "The few cases showing ice composition (top left of the panel, including also those around CI=1.5), ... represent the only ice PSCs detected during the whole season". I am confused by this statement, because the points near CI=1.5 lie to the right of the "ice" line and thus are classified as "STS/Mix" in Fig. 5b.

Yes, we need to specify this better. Following model simulations by Hoepfner et al. 2006, ice PSCs can fall also on the right hand side of the vertical line separating the ICE region to the STS/Mix region. Since the PSCs around the ICE-STS/Mix vertical line (and with NAT index above 0.8) have very similar spectral signatures, we further investigated background conditions (i.e. altitude, time, temperature) in order to understand whether they could be classified as ice. We here consider the analysis for the revised manuscript and report hereafter the details of 4 PSCs (they were 3 in the previous analysis) clustering together around the ICE-STS/Mix interface of the diagram:

1) LAT: 68.2, LON: -35.5, DAY: 3 Jan, CI: 1.30, CI1: 0.85, THETA: 691.8 K, TANG ALT: 28.9 km, T_ECMWF: 181.5 K, T_NAT threshold: 189.0 K

2) LAT: 75.4 , LON: -32.1 , DAY: 3 Jan, CI: 1.24 , CI1: 0.87, THETA: 631.0 K, TANG ALT : 26.1 km, T_ECMWF: 183.9 K, T_NAT threshold: 190.9 K

3) LAT: 68.1, LON: -17.3, DAY: 5 Jan, CI: 1.37, CI1: 0.84 , THETA: 631.0 K, TANG ALT: 25.0 km, T_ECMWF: 187.9 K, T_NAT threshold: 191.3 K

4) LAT: 72.0, LON: -31.5, DAY: 6 Jan, CI: 1.33, CI1: 0.82, THETA: 575.4 K, TANG ALT: 24.6 km, T_ECMWF: 186.3 K, T_NAT threshold: 192.8 K

The above values show that PSCs 1 and 2 have classification consistent with ICE (either below or on the 1.3 ICE-STS/Mix vertical line) and temperature 7 K below the T_NAT threshold. They can therefore be classified as ice. PSC 4 is very close to the 1.3 CI threshold, and has temperature 6.5 K below the T_NAT threshold which may still suggest an ice composition. PSC 3 has still cloud indices that could allow an ice composition although its temperature is only 3.4 K below the T_NAT limit. Notice that in the latter cases regions of colder temperature exists either directly above or below the PSC. All the 4 PSCs occur during the coldest temperatures reached by ECMWF during the whole season. Following the above details, and adopting a conservative approach as with the PSC detection, we classify as ICE composition PSCs 1 and 2, and as STS/Mix composition PSCs 3 and 4. The text was revised accordingly.

– p33204, L2-5: “The bulk of STS/Mix PSCs in the diagram was consistently associated with temperature below STS formation, and was observed between theta=450 and 550 K (Fig. 1b). At theta from 550 to 700 K, the STS/Mix PSCs had more scattered color ratio values and associated temperatures that reached values above TNAT”. It’s not clear to me how the reader is supposed to know at which altitudes the points plotted in Fig. 5 occur. This sentence points to Fig. 1b, but I do not see any easy way to associate the two figures. So while the reader can look at Fig. 5b and see that most of the STS/Mix PSCs formed at temperatures below TSTS, there is no way to judge that these occurred between 450 and 550 K, nor any way to tell that the points displaying greater scatter in Fig. 5b were located at 550 to 700 K.

To support the description and aid the reader, we introduced 4 new panels in Figure 5 reporting the tangent altitudes of MIPAS limb views that detected the PSCs. The 4 new panels are identical to panels a-d but with colors referring to altitude instead of temperature.

– p33204, L9-11: “The very high altitude clouds (CTH at theta > 700K) detected during the very first days of January had either a clear STS signature (see bottom right of Fig. 5b) or extended towards the ice region (top left of the panel)". I am skeptical that PSCs, especially low-CI (implying thick ice clouds) ones, formed between 700 and 950 K (i.e., up to _35 km altitude). In order for the authors to credibly assert PSC formation at such extraordinarily high altitudes, they need to provide far more support for their evidence than they do in this manuscript. There is no historical precedence for such high-altitude PSCs (see comment below about p33210, L4-7). What do CALIPSO measurements (or any other PSC observations, for that matter) show for January 2011? Do they provide any indication of PSC activity at these altitudes during this period? As I mentioned above, some statistics on the number of "false positive" PSC detections in MIPAS spectra are needed. It would indeed be a noteworthy discovery if MIPAS has detected Arctic PSCs up to 35 km, but the current presentation of the data has left me unconvinced that these signatures are real.

The paragraph was updated following the more conservative analysis performed for the revised manuscript. With the adoption of the more conservative detection method, PSCs were found up to 30.5 km (theta=750 K) altitude in 3 January 2012, in accordance with CALIPSO observations of PSCs extending above 30 km on 4 January above the same geographical region. The higher altitude PSCs previously reported (which reached 34 km, theta=950 K) did not withstand the more stringent direct scrutiny of MIPAS spectra. Some of these have therefore been removed from the sample and do not appear in the revised graphs. Please see detailed answers to reviewer #2 regarding this specific issue.

– p33204, L6-7 and L11-13: "suggesting PSC formation within cold mesoscale temperature perturbations that were not reproduced by ECMWF temperatures" and "In the clear STS cases, associated temperatures were largely above TNAT, again supporting the importance of mesoscale perturbations that were likely missed by ECMWF temperatures". I think more explanation and/or description of the ECMWF temperatures is needed, probably in section 2.4, which is labelled "ECMWF meteorological products" but which discusses only sPV (temperature is not mentioned in that section even though it is used in this study). I would have thought that the full-resolution ECMWF fields would capture mesoscale temperature fluctuations, so exactly what ECMWF data are being used should be specified. Particularly in the second case (L11-13), rather than supporting the occurrence of unrepresented mesoscale temperature perturbations, I think a more likely explanation is artifacts in the data that are being erroneously classified as PSCs.

We updated Sect. 2.4 with reference to ECMWF temperatures. We used the high resolution ECMWF temperatures on potential temperature levels. For both highest altitude PSC detections, associated temperatures where higher than the T_NAT threshold. Some shortages may arise interpolating the potential level needed for a specific PSC in regions where the low temperature was limited to a very thin altitude range. We added a comment to the revised text.

– p33205, L3-5: "Fig. 5e–h report some events with a remarkable consistency of the classified PSC and the expected NAT and STS regions also under very inhomogeneous conditions". Although good, I am not sure that the consistency of the PSC classes and STS/NAT formation regions can really be characterized as "remarkable", especially as some of the white diamonds do lie outside of the white contour lines. Also, I do not know exactly what is meant by "very inhomogeneous conditions" in this context. In the Arctic, low temperature regions are typically small, shifted off the pole, and not concentric with the vortex, so the conditions depicted in Fig. 5(e-h) do not look unusually inhomogeneous to me.

We tuned down our statement from “remarkable” to “very good”. Considering the inhomogeneous conditions of the samples we presented, the approximations involved in the method used to predict the composition of PSCs in different regions, the limitations of the method adopted to classify the PSCs, the analysis performed on the cloud top height, the classification appears to be in very good agreement with the regions of predicted PSC formation. We did not intend to claim conditions in 2011 were unusually inhomogeneous, but simply that they were inhomogeneous (at times, very inhomogeneous, as it often occurs in the Arctic). To avoid misleading the reader, we removed the term “very” in the revised sentence.

– p33205, L8 and L16-17: "each of the PSCs we classified as NAT was composed of at least 40% of such particles ... The overall fraction of 16% of NAT classified PSCs in the 2010–2011 Arctic winter can be therefore considered a good estimate of PSCs with dominant NAT composition". On p33199, the percentage of small NAT particles in PSCs classified as "NAT" was given as "30-40%". In either case, it seems like a fairly low percentage for such PSCs to be characterized as having a "dominant NAT composition". Also, it wasn’t clear where the overall estimate of 16% NAT PSCs during the winter came from, since the NAT fraction for the four individual periods was given as 10%, 20%, 18%, and 6%, respectively.
40% was a typo, it should be 30%-40% as reported in Sect. 2.3 from Hoepfner 2006. This estimate is based on the simulations performed by Hoepfner et al., at the basis of our analysis. It is the minimum density of NAT particles that will lead to the spectral signature classified as NAT. Despite agreeing with the reviewer the fraction is relatively low (one may expect at least 50% of NAT particles for such PSCs to be classified as dominant NAT composition), the classification is based on the presence in the spectra of the NAT signature. We therefore prefer to apply the method as originally published so as to be consistent with similar analysis performed on MIPAS PSCs. To our knowledge, this is the current state of the art for this classification.

The 16% fraction of PSCs with dominant NAT composition was calculated over the whole sample we analyzed for the Arctic 2010-2011 season: 472 NAT PSCs out of 2920 detections. When split into the 4 shorter periods, we found i) 33 NAT/320 TOT; ii) 52 NAT/ 266 TOT; iii) 374 NAT/ 2117 TOT; iv) 13 NAT / 217 TOT; and therefore leading to the percent values presented in the manuscript. The numbers have now been updated with the new analysis. These numbers have now been updated (see discussion above) by adopting the more conservative threshold of 1.8 for the CI above 30 km altitude, and adding the measurements taken with the middle atmosphere observation mode. The new statistics are slightly different, with the fraction of NAT PSCs over the total of detected PSCs now counting 12%, 21%, 19% and 6% respectively in the 4 periods, and 17% over the whole PSC season. The manuscript has been revised accordingly.

– p33206, L3-4: "The low HNO3 occurred where temperatures were below the STS formation threshold suggesting a capture by the NAT component of the STS/Mix PSCs". I didn’t really follow this argument – why would the temperatures being below TSTS necessarily imply that NAT particles in mixed clouds were taking up HNO3? Why wouldn’t there be HNO3 uptake by the STS droplets themselves?
NAT particles are known to have a much larger uptake of HNO3 as compared to liquid STS, so we assumed the larger than usual HNO3 reduction was linked to the NAT component of the STS/Mix PSCs. However, since there cannot be any direct evidence for a preferable HNO3 uptake by NAT particles, we removed “NAT component of the” from the sentence.

– p33206, L12-16: "As expected by the lack of ice PSCs, H2O (Fig. 2a) showed only a marginal decrease in late March in the lower stratosphere. ... the overall trend in H2O was consistent with the diabatic descent. The lowest H2O was reached in the second half of March at theta = 400-450 K, in coincidence with minima in HNO3 and O3, and increase in ClONO2". The analysis presented here suggests that very few water ice PSCs formed in this winter. Consequently, I doubt that the degree of dehydration was large enough to be discernible in vortex-average H2O abundances. In addition, even the few ice PSCs that were detected occurred earlier in the winter; no ice PSCs were still present in late March when the minimum in H2O was measured. It’s not clear how the fact that the mimima in HNO3 and O3 and the increase in ClONO2 occurred at the same as the minimum in H2O is relevant, since they all have different causes. Finally, even slow descent should have led to small increases, not decreases, in H2O in the lower stratosphere. So I think that the decrease in MIPAS H2O values in Fig. 2a needs to be explained better.
The sentence was removed.

– p33207, L13-15: "Snapshots ... reported in Fig. 3 show depletion of O3 occurring within the vortex since January". Since the first snapshot in Fig. 3 is from early February, it really cannot be judged from this figure exactly when O3 depletion started. Depletion that may have taken place between December and February would not be discernible in this figure. Moreover, chemical loss cannot be diagnosed merely through examination of "dot plots" of O3 such

as those in Fig. 3 in any case. Replenishment of O3 via diabatic descent could have offset any loss and made the O3 in these maps appear to be constant or even slightly increasing even though some loss took place.
The snapshots were included as additional information on the horizontal distribution of ozone and other tracers, besides the time evolution of the vortex averages shown in the previous figures. This is a commonly adopted approach in the literature, neglecting the impact of the diabatic descent in favor of a (see e.g. Manney et al. 2005 showing horizontal maps at 490 K throughout the Arctic winter). Indeed the snapshots on their own would not carry the needed information and are referred in the manuscript as examples of the horizontal distribution under the conditions of specific days. As compared to changes in ozone, the diabatic descent over the time span considered by the snapshots (of the order of 20 K per month) can be considered as secondary effect. We took care of underlying that our estimate are based on average values.

– p33209, section 4.1: This section compares the MIPAS measurements from the 2010-2011 winter to those from previous Arctic winters. Although I understand that the MIPAS data used in this manuscript differ from the IMK retrievals discussed in previous MIPAS papers on earlier Arctic winters, I think it would have been courteous for the authors to acknowledge that some of those papers exist (e.g., some studies by von Clarmann, Oelhaf, and others). In fact, stating how consistent the MIPAS2D data shown here are with previously-published results for some of these past winters might have been useful.
To our knowledge, there are no previous MIPAS-satellite papers dedicated to the overall evolution of the lower stratosphere ozone chemistry in one or more Arctic winters, beside Sinnhuber et al. 2011 (which we cite in the conclusions). Some of previous MIPAS-satellite papers focus on individual molecules, others use MIPAS for PSCs only, many on the upper Arctic stratosphere, whereas several comprehensive MIPAS studies focus on the Antarctic stratosphere. There are on the other hand studies performed with MIPAS on different platforms. This is the main reason why we did not cite any previous MIPAS paper, beside the overall Fischer et al. 2008 (which include all main papers up to 2008) and the main MIPAS PSC papers, and focused on the papers that were scientifically more relevant for the topic discussed here (and were not based on MIPAS data). We found it out of context to cite these other papers in this section. We agree it is appropriate to give reference to previous polar studies performed with MIPAS and preferred to include some references in the introduction, jointly with the other references on the instrument and while mentioning its suitability for polar stratospheric studies. As an example (beside the already cited PSC papers) we added:
1) Glatthor, N., von Clarmann, 25 T., Fischer, H., Grabowski, U., Ho¨pfner, M., Kellmann, S., Kiefer, M., Linden, A., Milz, M., Steck, T., Stiller, G. P., Mengistu Tsidu, G., Wang, D.-Y., and Funke, B.: Spaceborne ClO observations by the Michelson Interferometer for Passive Atmospheric Sounding (MIPAS) before and during the Antarctic major warming in September/October 2002, Journal of Geophysical Research (Atmospheres), 109, D11307, doi: 30 10.1029/2003JD004440, 2004.
2) Stiller, G. P., Mengistu Tsidu, G., von Clarmann, T., Glatthor, N., Hoepfner, M., Kellmann, S.,Linden, A., Ruhnke, R., Fischer, H., Lopez-Puertas, M., Funke, B., and Gil-Lopez, S.: An enhanced HNO3 second maximum in the Antarctic midwinter upper stratosphere 2003, Journal of Geophysical Research (Atmospheres), 110, D20303, doi:10.1029/2005JD006011, 2005..

Despite agreeing with the reviewer that an inter comparison of MIPAS data obtained with different retrievals would greatly benefit the scientific community, to date there are no comprehensive comparisons that we can cite in this manuscript. It is not common practice to compare to other retrievals within a specific analysis (each group adopts only their own data with no mention of how these compare to other datasets, e.g. to the operational one). A test on the internal self-consistency of MIPAS data obtained from different retrievals was discussed by Kiefer et al. 2010, as cited in the manuscript. A round-Robin inter comparison of all MIPAS retrieval algorithms is ongoing as part of ESA Climate Change Initiative on ozone and results will be available over the next one or two years.

Another overall comment on this section is that the second paragraph comes across as a collection of random thoughts with no thread tying the sentences together. It would benefit from some reordering and reorganization. For example, the paragraph starts off discussing temperatures and then moves on to descent. This is followed by a return to temperature in the sentence: "The cold core of the vortex traced by the TNAT region persisted until early April at a slightly higher altitude" (higher than what?). The TNAT sentence is followed by "Since in many past Arctic winters the vortex disappeared before spring, it should be noted that the distribution in time and the multi-year average of 2003-2010 data at a certain date is given by only those years for which the Arctic vortex was defined". This is a perfectly valid point to make, but one wonders why it is stuck in the middle of this paragraph. Immediately following this note, PSCs are discussed.
We revised the paragraph following the reviewer's suggestions.

– p33209, L11-12: "The figure also reports averages over the reference TNAT region (green) introduced in Sect. 3." I do not see the relevance of calculating averages over the TNAT region for most of these quantities. Perhaps for HNO3 the green line provides some useful information, but it is largely meaningless for the other products shown in Fig. 7. What would have been helpful to the reader would have been to show the multi- year average, which is referred to several times in the text but which is not depicted in any of these panels.

We agree the multi-year average is very useful and introduced it in the revised plots. Also, we removed the averages over the T_NAT region for most targets. We originally presented them to show possible biases between the cold pool and the overall vortex. We maintained the average over the T_NAT region for temperature (to show for how long the cold core was defined and compare to other targets), and for ClONO2 and HNO3. Both of these targets may suffer of differences between the cold core of the vortex and its edge. In particular, ClONO2 is shown to be restored much more quickly within the core than at its edge, as shown by the steeper change in its T_NAT-average. HNO3 appeared to be constantly slightly reduced within the core as compared to the outer part of the vortex. We revised the section accordingly.

– p33210, L4-7: "The altitude range covered by PSCs during 2010-2011 Arctic winter was also anomalous, with PSCs reaching altitudes above 30 km, as compared to maximum altitudes of 29km previously observed (e.g. Poole and Pitts, 1994; Fromm et al., 1999; Massoli et al., 2006)". As discussed above, I believe that the apparent MIPAS detections of high-altitude PSCs in January 2011 need to be validated before they are credible. In addition, I feel that the authors have mischaracterized the historical record in the sentence quoted here. The Arctic PSC sighting probabilities reported by Poole & Pitts [1994] drop to zero above about 26 km. Similarly, Fromm et al. [1999] report no Arctic PSC observations above about 25-26 km. Thus neither of these references can be used to support the assertion that PSCs have been observed in the Arctic up to 29 km. Finally, although PSCs in the Ny-Alesund record mainly appeared in the altitude range 20-24 km, they have been found as high as 28 km [Massoli et al., 2006] (but not 29 km). Thus the MIPAS detections of PSCs as high as 32-35 km are indeed "anomalous", and, I would argue, dubious (at least until backed up by correlative measurements).

Please see answers to previous comments on PSC detection and our adoption of a more conservative threshold for PSCs. In our original manuscript, we used the cited references to underlying how unusual our detections above 30 km altitude were, citing the papers as proof that none of the observations considered in those studies reached altitudes above 29 km. We agree with the reviewer Poole&Pitts [1994] PSC sighting probability drops below 0.01 above 26 km, altitude which also corresponds to the highest detected PSC in the study by Fromm et al. [1999]. We also acknowledge we were misled by the usage of 2 km altitude bins in the climatology by Massoli et al. [2006], whose highest bin is centered at 28 km altitude but reaches the 29 km altitude we quoted. As pointed out by the reviewer, in the text they state they found PSCs between 16 and 28 km altitude. We revised our text accordingly. In the revised text, we also specified that Poole&Pitts [1994] and Fromm et al. [1999] underline how unusual the 30.2/30.5 km altitude PSC we observe is. With our revised analysis (which led to discard about 38 PSC detections out of 2920, mostly all at high altitude), we found 86 detections above 25 km altitude (60 above 26 km altitude) in the nominal mode out of 2882. This corresponds to about 3% of detections above 25 km altitude, as compared to the 1% around 25 km altitude (and less above) in Poole&Pitts [1994] climatology. Considering that their climatology was based on 12 years of measurements which include also warm years, our high altitude results can be considered consistent.

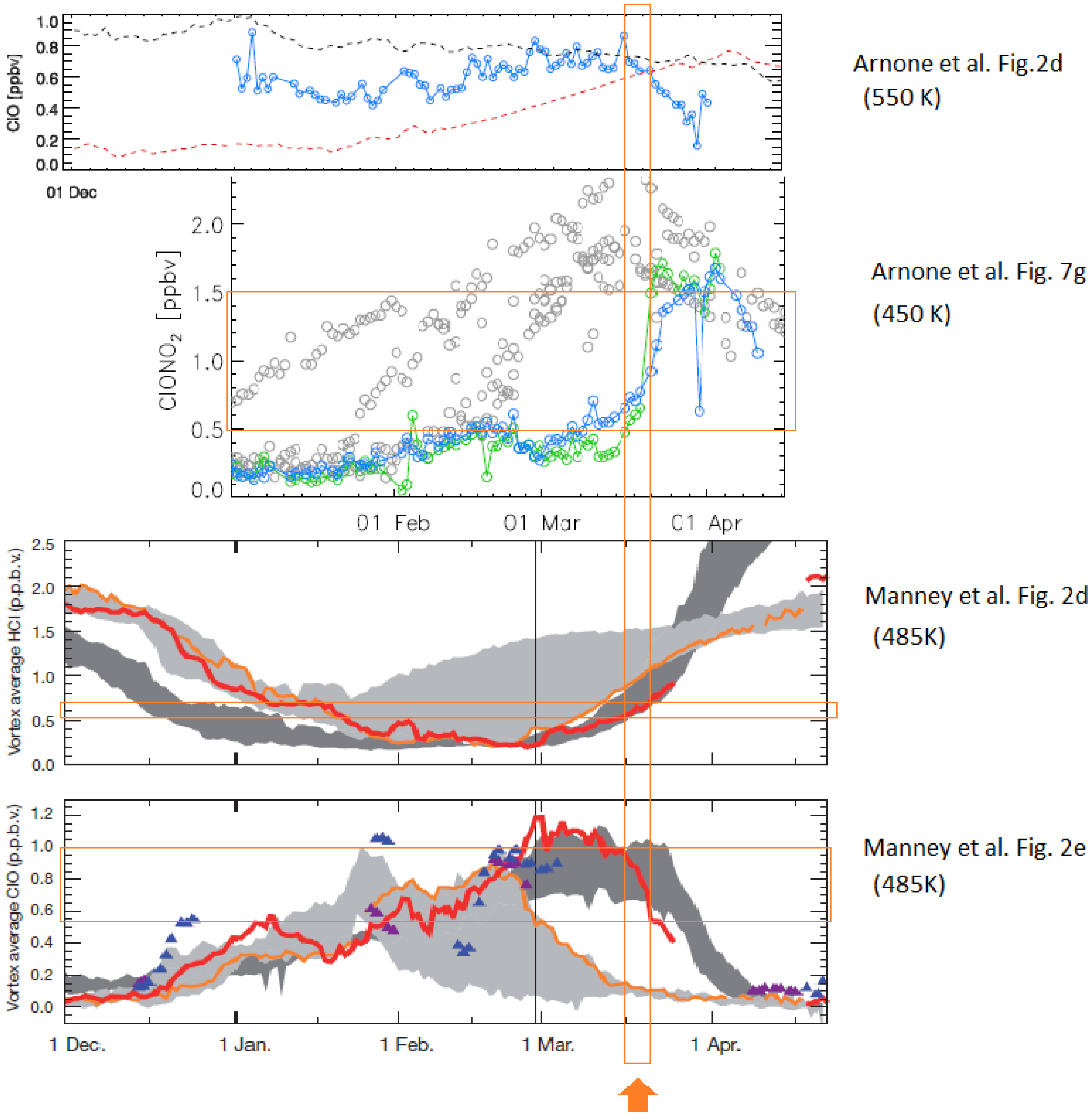
– p33210, L17-20: "NO2 at theta=450 K (Fig. 7f) showed sporadic very high values, likely associated with evaporating PSCs, more often than previous years. In particular, the highest NO2 values were reached in the last week of February consistently with a period of minimum HNO3". I don't follow this argument. For one thing, 450 K seems a little high to me for renitrification, which has typically been reported at significantly lower altitudes. Also, wouldn't evaporating PSCs lead to *higher* HNO3, not minimum values? The largest NO2 peaks occurred at the end of February / beginning of March, which seems too early in the season for there to have been substantial HNO3 photolysis on such a rapid timescale.

We updated the sentence further mentioning that the two periods (late January and late February) with high NO2/low HNO3 were coincident with a relative reduction in the PSC coverage which followed a prolonged period of extensive PSC coverage. In both cases the increase in NO2 follows the disappearance of PSCs at low altitude, however further detailed investigation is needed to explain this.

– p33210, L22-26: "... the period of highest ClO (see Fig. 2d) which reached its peak value on 15 March. ClO then started to be converted into its reservoir ClONO2, as shown by the prompt increase of ClONO2 reaching previous years values. This prompt ClONO2 change points to reconversion of ClO into ClONO2 reservoirs rather than (or concomitant to) HCl". I do not agree with the interpretation that the MIPAS data suggest reconversion into ClONO2 rather than HCl. Fig. 7 shows that ClONO2 in 2011 did not actually reach previous years' values during the initial phase of chlorine deactivation. Although the ClONO2 values matched those in other years in early April, in previous

winters that time period was well after the initial deactivation phase, at a stage when chlorine is being slowly repartitioned from ClONO2 into HCl (note the decrease in the values of the grey points between mid-March and early April in all other years). In contrast, in 2011 deactivation did not get underway to an appreciable degree until mid-March, as the authors note and Fig. 2d shows. Considering the strong ClO enhancement in 2011, had formation of ClONO2 been the primary initial deactivation pathway, ClONO2 values would have been very high by early April, similar to the highest values observed in mid-March in some other years. On the basis of Aura MLS measurements, Manney et al. [2011] argue that HCl reformation played a greater role in the chlorine deactivation process in 2011 than is typical for the Arctic, and to me the results of Fig. 7 support that argument. I think it is clear that, unlike in typical Arctic winters when ClONO2 reformation dominates, in spring 2011 both chlorine reservoir species were playing important roles.

We further compared our ClONO2 data to Manney et al. 2011 data. In Figure_reply 3 we report a crude inter comparison of the plots of ClO and ClONO2 from our analysis with ClO and HCl from Manney et al. [2011]. Please note that because of the limitations in our ClO retrieval (as explained in the data section), we had to plot ClO on the 550 K isentrope. Or ClONO2 at 450 K is therefore more closely comparable to the ClO (and HCl) at 485 K from Manney et al.



Figure_reply 3 – comparison of ClO and ClONO2 from our manuscript to HCl and ClO from Manney et al. 2011 (top to bottom, see labels).

As discussed in our manuscript, when ClO started to deactivate in mid-March, ClONO2 showed a prompt change both in the vortex average (Figure_reply 3, second panel from the top, blue curve), and in the T_NAT average (green curve). More precisely, the abrupt change was within the cold pool of the vortex as shown by snapshots in Figure 4 of our manuscript (compare 8 and 28 March), whereas the outer vortex had already high ClONO2 values. We highlighted this 5-day period of abrupt change by a vertical orange rectangular shape throughout the 4 panels of Figure_reply 3. ClONO2 in the inner vortex increased by a factor 3 from 0.5 to 1.5 ppbv (up to previous years vortex average values). As a consequence of this abrupt change being concentrate in the inner part of the vortex, the vortex average ClONO2 responded more moderately (compare blue and green curve), increasing from about 0.65 to 0.9 ppbv (about +0.25 ppbv), and up to 1.35 ppbv in 2 more days (i.e. +0.7 ppbv in 7 days). During the same 5-day period ClO at 485 K (bottom panel) dropped from about 1.0 ppbv to 0.55 ppbv, i.e. about -0.45 ppbv, and was almost stable over the next two days. HCl (third panel from top) increased by 0.15-0.2 ppbv over the 5-day period, by 0.2-0.25 ppbv over the 7-day period. If we use these average values, the main pathway of deactivation was ClONO2 over the 5-day period (but closely followed by HCl, with roughly a 60/65 to 40/35% partitioning), and even more dominantly over the 7-day period (where then the partitioning becomes 75 to 25%), as we claimed in our manuscript. Over this slightly longer period, and following weeks, one needs to consider that slow conversion of ClONO2 into HCl was already occurring because of the late season (so that a component of the background steady increase of HCl came from ClONO2 rather than ClO). This despite 2011 being very close to Antarctic conditions. The difference in the various components of the chlorine partitioning may likely be dependent on the slightly different isentropes used for the averages, and possibly also by the lack of MLS data close to the North Pole (where certainly ClONO2 had a major change – see snapshots in Figure 4 of our manuscript). A model should be used for a more precise evaluation avoiding further interpolation of observational data. A further consideration on the HCl behavior arises from the steepness of the HCl increase around the 5-day period we highlighted and following weeks. Although 2011 HCl matches the envelope of Antarctic conditions, its steepness appear the same as 2005, with a marginal increase of its saturation value at the end of April (when MLS measurements were available again) as compared to previous Arctic springs. The lack of MLS data during the recovery phase may be misleading: The difference of 2011 HCl in comparison to previous years appears to be due much more to a delay in its raise rather than a change of its behavior (e.g. steepness of increase, absolute values reached) towards Antarctic conditions.

Joining together the above considerations and the reviewer's comments, we believe there is evidence to support an abrupt reconversion of ClO into ClONO2 over the third week of March (particularly in the inner part of the vortex) with a partitioning of the order 60/75% to 40/25% between ClONO2 and HCl, together with a slower reconversion into HCl, then followed by a decrease in ClONO2 and continue increase of HCl over the following weeks. Even though conditions were as close as Antarctic ones as ever before (for example with HNO3 almost reaching previous years Antarctic values – see Figure 2a Manney et al. [2011]), both the plots replicated in Figure_reply 3 and the average ClONO2 profiles in Figure 8 of our manuscript, support a much more Arctic-like behavior of the 2011 Arctic conditions. In the sentence questioned by the reviewer, "This prompt ClONO2 change points to reconversion of ClO into ClONO2 reservoirs rather than (or concomitant to) HCl", the use of "rather than" expressed the above considerations. We further tuned the sentence stating ClONO2 was the main pathway (especially in the inner part of the vortex) and that HCl was present but not closer to Arctic conditions than to Antarctic ones, so as to avoid to mislead the reader. These considerations on chlorine reservoir partitioning were updated also in the other relevant parts of the manuscript.

– p33211, L1-19: I have several comments on this paragraph. "These characteristics are similar to the behaviour of the Arctic vortex in the winters 1995-1996 ... 1996-1997 ... and 1999-2000". What specific characteristics are similar? Depending on what is meant by that statement, 1996/1997 may not belong on the list, since it was not a particularly cold winter until very late in the season, and chlorine activation and consequent ozone loss were not as severe or extensive as in the other years. In the statement "although this winter the O3 reduction in the lower stratosphere (theta=450-500 K) was deeper and more broadly extended", does "deeper" refer to altitude (which wouldn't make sense, since the sentence is specifically referencing a narrow theta range) or magnitude? Similarly, does "broadly extended" refer to the vertical or horizontal direction? In 2004/2005, it is stated that the PSC season "was halted in mid February". The authors need to provide a citation for that, because I think that the PSC season lasted into late February at least; certainly chlorine remained activated at some levels in the lower stratosphere into early March (as the Santee et al., [2008] paper they reference shows). In the statement "the delayed reconversion of ClO into the ClONO2 reservoir", I would delete "ClONO2" and change "reservoir" to "reservoirs", since, as mentioned above, HCl also played a role. Again, what exactly does "wider PSC coverage" mean – horizontally or vertically or both? The authors note that "The observed 2011 denitrification appears to have had a greater role than in previous years". They do not need to speculate, since Manney et al. [2011] (which should be

cited in this sentence) demonstrated conclusively that the more severe denitrification in 2011 partly contributed to the greater ozone loss.
Finally, they state that "the much larger fraction of STS/Mix PSC observed suggests their active contribution in driving the lower stratospheric chemistry". The recent WMO report pointed out that chlorine activation primarily occurs on liquid aerosols, especially in the Arctic, and it would be good to reference that report here also.
The section was updated following the reviewer's comments.

– p33212, L3-5: "Averages were performed on pressure levels and respectively over the 75-90 N and 75-90 S geographical latitude, so as to reflect both chemical and dynamical changes of the vortex". I am not sure why the authors would consider mixing together the effects of chemical and dynamical processes, as is done in these broad latitude band averages, to be helpful in comparing the two hemispheres. I would think that would just complicate the comparisons and render them much less meaningful, especially since the Antarctic vortex is much larger (and generally more symmetric with latitude) than the Arctic vortex.
Since the rest of the analysis was performed on conditions within the vortex, we here present a comparison of the average conditions of the two hemispheres. To avoid misleading the reader, we updated the paragraph specifying the inter-hemisphere comparison. This is similar to the averages over geographical latitude bands presented e.g. by Manney et al. 2011 Nature, supplementary material, Figure 1. From a chemistry perspective at regional scale, we agree it is best to look at conditions within the polar vortex. From a global scale perspective, and in order to easily understand what the impact of this year can be as compared to the impact of typical Antarctic ozone hole conditions, also the size and movements of the vortex should be considered (indeed a very small Arctic vortex may bury an interesting chemistry at regional level but will have a very limited relevance at global scale). We therefore believe it is instructive to look at inter-hemispheric differences over a fixed geographical region and prefer to maintain them for this additional comparison.

– p33212, L20-29: I have several comments on this paragraph. First, the authors state that: "Arctic O3 reduction in 2011 was less pron[o]unced than in the Antarctic, associated with much weaker denitrification and absence of dehydration below 20 hPa". Dehydration has no bearing on the severity of ozone loss. Second, the statement is made: "At 20 to 10 hPa (which corresponds to the theta=650-800 K in the middle stratosphere discussed above) most Arctic parameters are well in agreement with Antarctic conditions, with the exception of a different partitioning of the nitrogen family (see NO2 and HNO3 ...". Certainly ClONO2 needs to be added to this list, and I would argue that agreement with the Antarctic is not very good over the 10-20 hPa range for O3 or altitude either. As discussed above, I take issue with the statement: "In the lower stratosphere, the 2011 Arctic winter ClO was largely deactivated into ClONO2, so that the latter reached higher values in March as compared to Antarctic conditions". Although the statement is generally true, this is always the case in the Arctic, and Fig. 8 shows that from about 40 to 100 hPa the ClONO2 was considerably lower in 2011 than in previous Arctic winters. Thus I feel that "at the expenses" should be edited out of the sentence: "2011 maintained ClONO2 as a channel for ClO deactivation (at the expenses, or together with HCl ...".
The section was updated following the reviewer's comments. Regarding the similarity between the Arctic in 2011 and Antarctic conditions, we replaced "well in agreement" in the sentence with "close to", and still point most parameters are in agreement at 10-20 hPa (specifying O3 is in agreement at 10 hPa): the altitude in this case is much more close to Antarctic conditions than to any other previous Arctic year. For the ClO reservoirs, we followed the reviewer's comments and the considerations made above regarding the ClO reservoir partitioning (i.e. pointing out that the main deactivation pathway was ClONO2 and that HCl contributed to a lesser extent). ClONO2 was closer than ever before to Antarctic conditions, but still closer to previous years Arctic values than to the Antarctic ones.

– p33213, L18-19: "In the middle stratosphere (theta=700-850 K) O3 was depleted by 25% down to 3.3 ppmv, at the lower edge of the 2003-2010 range". Although this high-altitude ozone depletion was noted in the O3 subsection (3.3.3), the comparison to previous Arctic winters was not discussed in the relevant section (section 4.1), so it seems odd to mention it in the Conclusions. In addition, since this depletion is presumably caused by gas-phase processes (not heterogeneous chemistry on PSCs), do the authors have an explanation for why it is larger in 2010/2011 than in most other years?
We have included a mention of this in Sect. 4.1 too. We do not have a specific explanation for this behavior, but believe it is of interest to mention also that depletion in the 2011 Arctic winter middle stratosphere was extraordinary.

– p33213, L26 - p33214, L1: In discussing the Manney et al. [2011] results based on Aura MLS measurements, the authors state that: "Comparison of our ClONO2 to their HCl trends suggests 2011 Arctic ClO deactivated into ClONO2 rather than HCl which did not show the same prompt increase". I think the authors are misinterpreting the MLS results slightly. Manney et al. did not argue that HCl was the only or even the primary reservoir to be formed

during chlorine deactivation in 2011 – only that it played a bigger, more Antarctic-like role than in typical Arctic winters.
We have updated the comment on Manney et al. results following the reviewer suggestion and the consideration made above on the chlorine reservoir partitioning.

– Overall comment on the Conclusions: Since this manuscript was submitted, another paper on the 2010/2011 Arctic winter has been published [Sinnhuber et al., GRL 38, L24814, doi:10.1029/2011GL049784, 2011]. The authors may not have known about this GRL paper prior to its publication, but since it is also based on MIPAS measurements (albeit from a different retrieval), I really feel that some mention of how their results and these agree is necessary.
Yes, we agree the newly published paper should now be mentioned. We have included a sentence also on Sinnhuber et al. 2011.

– p33224, Fig. 5: I do not understand the relevance of the insolation snaphots provided in Fig. 5(e-h). The information on PSC classification relative to the potential NAT/STS formation region is of some interest, but the average number of sunlit hours for these particular dates seems almost completely irrelevant. Evidently the authors did not find these maps of daily sunlit hours particularly useful either, as they did not discuss them at all in the text of the manuscript.
The insolation maps were removed from the snapshots.

Typos and other minor wording issues:
– throughout the manuscript: "associated to" should be "associated with".
OK.
– p33193, L16: The term "lowermost stratosphere" has a specific definition (the region between the tropopause and the 380 K isentrope) that I do not believe is the intended meaning here. It should just be "lower stratosphere".
OK.
– p33194, L22: "anomalies induced by horizontal gradients adopting 1-D codes". "adopting" is not the right word here. Perhaps something along the lines of "anomalies induced by horizontal gradients not accounted for by conventional 1-D codes" would be better.
OK.
– p33195, L4: It would be better to change "injected" to "inserted" and "angle" to "inclination".
OK.
– p33196, L8: "assumes the atmosphere homogeneous" should be "assumes that the atmosphere is homogeneous".
OK.
– p33198, L10-11: "into MIPAS spectra" should be "in MIPAS spectra".
OK.
– p33199, L7-8: "because of their spectral signature similar to" should be "because of the similarity of their spectral signature to".
OK.
– p33199, L26: "adopted vortex averages" should be "calculated vortex averages".
OK.
– p33200, L18: "fraction to" should be "fraction of".
OK.
– p33201, L1-2: "Only when PSCs disappeared in March the regions of deepest O3 depletion around _=450 K could be fully observed" should be "Only when PSCs disappeared in March could the regions of deepest O3 depletion around _=450 K be fully observed".
OK.
– p33201, L18: "centred" should be "pole-centred".
OK.
– p33201, L21: "monotonical" should be "monotonic".
OK.
– p33203, L17: "panels f to i" should be "panels e to h"
OK.
– p33203, L20: "Beside" should be "Besides".
OK.
– p33203, L23: "largely scattered" – it would be better to say "highly variable".
OK.

– p33203, L20 to p33204, L29: This is a very long paragraph. It would be easier for the reader if this discussion were broken up into separate paragraphs for periods (i), (ii), and (iii, iv).
OK.
– p33204, L18: "82% PSCs" should be "82% of PSCs".
OK.
– p33204, L24-25: "scales" should be "scale" and "continue" should be "continuous".
OK.
– p33204, L26-27: Since for all of the other periods the STS/Mix percentage was stated first, and then the NAT percentage, it would be better to keep that same ordering for period (iv) as well.
OK.
– p33205, L9: "On the contrary" should be "In contrast".
OK.
– p33205, L22-23: "As a result of PSC formation, HNO3 was significantly removed from the lower stratosphere from January to April". I think it is necessary to add "and subsequent sedimentation of PSC particles" after "PSC formation", since PSCs form in all cold Arctic winters, but rarely is HNO3 actually removed from the lower stratosphere through denitrification (usually it is returned to the gas phase when the PSCs evaporate).
OK.
– p33205, L26: "successive" should be "subsequent".
OK.
– p33206, L23-25: "activation of ClO in the vortex, reaching a maximum in mid-March (Fig. 2d). This is also shown by ClO data at theta=550 K". This wording is slightly confusing because Fig. 2d also shows ClO at 550 K.
To avoid misunderstandings we moved "theta=550K" to the previous sentence.
– p33206, L25: "ClO sporadic high values" should be "sporadic high ClO values".
OK.
– p33207, L10: "showed a very stable O3" would read better as "showed little change in O3".
OK.
– p33207, L20-23: "Comparison ... results in a chemistry-driven depletion" should be "Comparison ... results in an estimate of the chemistry-driven depletion".
OK.
– p33208, L19: "a relative flat distribution" should be "a relatively flat distribution".
OK.
– p33208, L25-28: "Loss of HNO3 severely acted on a fraction of vortex air in late January and February, with greatly scattered low values, especially after the SSW on 3 February. It then turned into a more homogeneous low vortex HNO3 in March and early April". I'm not sure what "severely acted on" means, maybe "had a large effect on"? Also, why only "a fraction of vortex air"? The last sentence would be better written as "Low HNO3 was then more homogeneously distributed in the vortex in March ...".
OK.
– p33209, L22-23: "the weak ascending trend observed in typical years". It would be clearer to say "ascending trend observed in H2O in typical years".
OK.
– p33210, L11: "persisting also when temperatures rose" would be better as "and remained low when temperatures rose".
OK.
– p33212, L5: "Next to" should be "In addition to".
OK.
– p33212, L19-20: "Beside very similar ..." should be "Despite very similar", "pronunced" should be "pronounced", and "associated with" should be "consistent with".
OK.
– p33213, L16: "84% PSCs" should be "84% of PSCs".
OK.