Response to Review 2

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We thank the referee for her/his thoughtful comments and suggestions for improvements. We revised the manuscript critically and think that the manuscript has significantly improved after the comments and suggestions have been considered.

In the following, we respond to individual comments. Original remarks of the referee have been enclosed in quotation marks, using an *italic* font. Responses are given below each comment and are marked by "Answer" in a ***bold italic*** font.

Major comments:

"Most of your discussion is based on the Arctic winters 2010/2011 and 2009/2010. Therefore, the broad title on the comparison is not appropriate. You can use something like "Chemical ozone loss and ozone mini-hole event during the Arctic winter 2010/2011 "."

Answer: We agree to the referee and changed the title according to your suggestion in "Chemical ozone loss and ozone mini-hole event during the Arctic winter 2010/2011 as observed by SCIAMACHY and GOME-2."

"You also need to clearly state the focus of this study; Arctic ozone loss or the ozone mini-hole event? You could dedicate two separate sections for these (e.g. Introduction, data and method, Arctic ozone loss in 2011, Ozone minihole event in January 2011, and conclusions) instead of the subsections (e.g. Section 3.8)."

Answer: We thank the referee and we will revise carefully the structure of our manuscript.

Before we comment the structure of the paper, please let us the state the following: The overarching aim of our manuscript is to report correlative observations of the unexpected severe ozone losses over the Arctic in winter-spring 2011 from the SCIAMACHY instrument. The instrument's capability to measure in limb mode made it possible to focus on the presentation of vertically-resolved observations with a good spatial and temporal resolution. In the limb time-slices we found a remarkable structure that has not been reported elsewhere and required a detailed investigation. Apart from that, we, of course, compared to other techniques (nadir and occultation mode observations, CTM).

When we prepared the manuscript, we discussed several times what the best structure would be to adequately present our message. In this discussion we also examined other possibilities to structure the paper, for instance a more dedicated one, putting the different results in equivalent main results sections.

When it became clear which results will be considered and in which presentation form (e.g. maps, time-slices etc.) we concluded that:

1) A more general manuscript structure (Abstract/Introduction/Methods/Results and Discussion/Conclusion) is the most appropriate to present our message. We agree to the referee that the manuscript has a deficit in highlighting the main message, so that the focus is not becoming clear enough. We will carefully reexamine this issue.
2) Our main objection against a dedicated manuscript structure, containing several equivalent result sections, was that it would have implied numerous repetitions and cross-references to other sections.

Basically, in its current structure the manuscript's "Results" section divides the different approaches and techniques into clearly separated parts (i.e. separated sub-sections). We made sure that parts providing a similar scientific content (e.g. time-slices, different chemical species) are structured in a similar manner (e.g. first describing O3, followed by BrO and NO2), to make it simple for the reader to compare between respective sections. The different result sections are connected by cross-references whenever it is

appropriate - but we use cross-references in well-aimed portions in order not to confuse the reader, or to force the reader to jump between the different parts of the manuscript.

With the separate ozone mini-hole sub-section at the end of the manuscript, we are quasi zooming into the main results (limb measurements and their evaluation) shown and discussed before. Since our main aspect here was to examine whether it is an artefact of the approach used to infer chemically ozone losses from limb-observations or not, and also in order to explore which importance or significance this event may have, we decided 1) go a little more into detail here and 2) to place the part on the end of the results section in order to better highlight what we found out.

A side aspect has to be mention here also: In the ozone mini-hole analysis, discussions about the general meteorological situation played a much larger role than in the other sub-sections describing results, so that a discussion of this issue within one of the other sub-sections did not made any sense, and would certainly confuse the reader.

"*Since this is a scientific manuscript, you could avoid the history of satellite launching or preparation things. In addition, there are several publications on SCIAMACHY and GOME observations. Therefore, Sections 2 and 3 can be cut short substantially with relevant validation references and by removing the figures 3 and 4. Describe only what it matters here (your data processing for this study). The figures 3 and 4 do not give any additional information about the chemical ozone loss. Furthermore, it is strange to describe the meteorological situation of the winters with ozone maps (instead of temperature or potential vorticity maps). On the other hand, Hurwitz et al. (2011) and Kuttippurath et al. (2012) have given detailed descriptions of the dynamics of these winters. You need to cite these publications here.*"

Answer: The referee refers to several aspects here. To answer this point, we first separate the various issues and discuss individual aspects in more detail below:

Following issues are addressed by the referee:
1) History and overview of the satellite instruments used in this study.
2) Publications about SCIAMACHY and GOME/2 observations.
3) Length of Methods section (Section 2), describing all approaches used in our analysis.
4) Length of Results section (Section 3).
5) Relevance of Figures 3 and 4 with respect to chemical ozone loss.
6) Examination of a particular situation by means of polar stereographic maps of different parameters.
7) Missing/suggested references.

Comments to individual points 1-7:

1) We agree to the referee and rewrite the preface to the "Methods" section, compacting the message.

2) Referring to 1), we reduce general instrumental descriptions to a necessary minimum.

3) In the "Methods" section of the manuscript, we provide the reader with all relevant informations in brief, along with appropriate references to articles describing the individual methods and their evaluation. We believe, it is not making much sense to short these sub-sections substantially because then this section turns more or less into a list of references, which is insufficient. A limitation here would also strongly limit the interpretability of the measurements and modelling results. In this respect, we like to point out that also readers without in-detail knowledge of satellite remote sensing may be interested in this paper. In the current manuscript, we, in particular, tried to address their specific demands to find also a little "extra-information".

4) Here, the same comments generally apply as those concerning the paper structure with respect to the ozone mini-hole (see above; "*You also need to clearly ... the subsections (e.g. Section 3.8).*"). To keep it short here, we refer to our comments made above.

5) We do not completely agree. In particular, Fig. 4 underpins an integral part of our analysis because it is clearly demonstrating that substantial differences can be seen in the three ozone data sets, depending on the methods used for their constructions. Without this figure, important relationships on winter-spring time Arctic ozone could not be represented in equivalent clarity and quality. Because in Fig. 4, and in its

associated discussion in Section 3.2, we show that small differences in the definition of the "stratospheric columns" may affect the total ozone distribution. And that model assumptions on the vertical transport can substantially affect the inferred total ozone value although it may have only a small impact on the vertical distribution of ozone. And knowledge about those fundamental relationships between the different data sets is essential in order to ensure comparability of the results shown in more detail later in the manuscript.

Fig. 3, however, does not provide a direct information about chemical ozone losses, insofar we agree to the referee. But on the other hand, Fig. 3 provides very helpful informations to the reader who is not familiar with the topic. Note, Fig. 3 is not a reproduction of the respective Fig. of Weber et al. (2011), instead it is an update - now also considering the observed severe 2011 ozone losses, putting them into context to recent years, respective situations over Antarctica, and shows the linkage to the major driver of ozone variability, the established relationship to planetary wave activity. Therefore, it is beneficial to consider Fig. 3 to better understand the paper's central message.

6) When we interpret this point correctly, the referee refers to figures used in the analysis of the ozone mini-hole event (Section 3.8). If this is correct, we like to state the following: Our aim is to provide a consistent picture of this event. Off course, a tracer distribution map cannot be a source to describe a meteorological situation. But this was also not our aim. There are several aspects we like to mention here: A) The total ozone maps (Fig. 11 and 12) are used to demonstrate the temporal evolution of Arctic ozone on a daily base (in terms of observations and their reproducibility by a CTM). We used these maps to see how a specific, synoptic scale event evolves within the vortex. Here, the daily maps are exactly fitting our needs. B) In the subsequent figures (Fig.13-15) we describe the associated meteorological situation by means of polar stereographic maps of near tropopause temperature, geopotential height, and pressure. We do not show meteorological parameters on other levels nor using other presentation formats because the key in understanding the causes of low ozone in January 2011 are lying at the tropopause over the Asian landmasses. This is also clearly stated in the manuscript. However, we will revise Section 3.8 carefully, in order to improve the message.

With respect to the criticised use of tracer distributions in order to assess a meteorological situation, it should be also mentioned here, that tracer distributions are very frequently used to assess certain stratospheric phenomena that are associated with meteorology, e.g. SAO / QBO relationships (e.g. Baldwin et al., 2001), or Rossby wave breaking induced lateral mixing from polar vortices into mid-latitudes - a phenomenon that is very similar to the origin of ozone mini-hole formation.

7) When we understand this point correctly, it is also referring to point No. 6 above, addressing the use of ozone maps to illustrate a specific meteorological situation in mid-winter 2011. Therefore, the answer to this point, can only be given in a similar context. The referee suggests to consider two additional studies addressing the dynamics of the 2011 winter. In the following we discuss how relevant they are for our work, and in particular for Section 3.8, exploring the ozone mini-hole condition:

The study of Hurwitz et al. (2011) addresses meteorological aspects leading to severe ozone losses in 2011 by highlighting the importance of the state of subarctic sea-surface temperatures (SSTs) and the Pacific-North American (PNA) circulation pattern, additionally to the already established weak interference of the vortex by the quasi-biennial oscillation (QBO) and Central Pacific SSTs (La Nina).

In their analysis this relationship has been shown by differences in the zonal mean March temperature, relative to the others years since the beginning of global monitoring by satellites. However, Hurwitz et al. (2011) do not refer to any low ozone measurements during mid-winter 2011, nor show the temporal evolution of stratospheric Arctic ozone. Therefore, this study is not appropriate to explain a specific behaviour during mid-winter 2011, we discuss in more detail in section 3.8.

But in general we agree to the referee, and state that Hurwitz et al. (2011) has to be considered in the revised manuscript (as it was already the case), because it is an important study about the exceptional nature of 2011 Arctic ozone levels. We will cite it appropriately.

With respect to the study of Kuttippurath et al. (2012), we like to state that we did not had knowledge about its existence during time of manuscript preparation. We will cite it in the revised manuscript.

"Section 3.5.1: This section explains that there are large differences between modeled and measured trace gases presented in the manuscript. The differences are not well explained too. Although the link between these trace gases and ozone loss is well known, the link is missing in the text. Since you can explain the ozone loss process and its relation to the species with measurements, you do not need this section at all, as it raises more questions."

Answer: We thank the referee to point this out. We agree that the relationships between the tracers should be better described in this section. But with respect to shorten the consideration of our modelling works to a single figure showing ozone losses for the two years only, we believe it is better to provide a more complete picture here and show also the time-series of O3, BrO, and NO2. Because we think it is valuable for the reader to find the measured trace gases systematically evaluated and discussed with respect to their reproducibility by a CTM. Although ozone is pretty well simulated by the model (this is not new - certainly this also applies for the majority of other CTMs), our works clearly exhibit that relationships in modelled trace constituents exists that were not expected, may be not fully understood, and may be not adequately described in other studies on comparable subjects. In particular from a modeller's perspective, it is very constructive to find also discrepancies between models and observations described, although this may raise questions. We will carefully revise the section.

"Section 3.7: Compare the timing and extent of chlorine activation with other published results for this winter."

Answer: The referee correctly points out that we compared our analysis of chlorine activation to two other studies only (Manney et al., 2011; Khosrawi et al., 2011). We will revise the section and consider additional studies.

"Section 3.8: In this section you state that (a) the size of OMH was atypically large, (b) its direction was opposite to what was observed previously, (c) was not associated with large tropopause elevations, (d) and its life cycle was unusually large (more than 10 days or so). In addition, at some places you use "OMH-like situation" (e.g. page 16632, 16633), but in other places, including conclusions, you use OMH (e.g. page 16636). In general, all these statements are strong enough to refute the event to be called an OMH. Therefore, please make your arguments stronger to establish the low ozone episode was an OMH (if this is your point). "

Answer: We agree to the referee. In the revised manuscript we will introduce an identical notation for the situation when ozone was low in January 2011. We also understand the referee's objection to call this situation an ozone mini-hole (OMH) and will refer to as "OMH-like situation".

Specific comments:

"Page 16600, lines 1, 2: There are two significant publications on ozone loss in 2010. They should be cited and discussed (Kuttippurath et al. 2010, Wohltmann et al. 2013).

> *Wohltmann, I., Wegner, T., Müller, R., Lehmann, R., Rex, M., Manney, G. L., Santee, M. L., Bernath, P., Sumińska-Ebersoldt, O., Stroh, F., von Hobe, M., Volk, C. M., Hösen, E., Ravegnani, F., Ulanovsky, A., and Yushkov, V.: Uncertainties in modelling heterogeneous chemistry and Arctic ozone depletion in the winter 2009/2010, Atmos. Chem. Phys., 13, 3909-3929, doi:10.5194/acp-13-3909-2013, 2013.*
>
> *Kuttippurath, J., Godin-Beekmann, S., Lefèvre, F., and Goutail, F.: Spatial, temporal, and vertical variability of polar stratospheric ozone loss in the Arctic winters 2004/2005 - 2009/2010, Atmos. Chem. Phys., 10, 9915-9930, doi:10.5194/acp-10-9915-2010, 2010.*

Answer: We agree and consider the studies.

"Page 16608, line 11: Any reason for selecting this particular value as vortex edge?

Answer: We are following Nash et al. (1996) and define the vortex edge based on PV and chose a value of 38 PVU at the 475K isentropic surface as boundary. This is consistent with the treatment of Sonkaew et al. (2013), who examined ozone losses in the Arctic stratosphere during the first seven years of SCIAMACHY limb observations, also applying the method of Eichmann et al. (2002). We have to mention here, that the referee's comment also affects manuscript page 16607, line 18-20, where we first mention the criteria without citing respective studies. This is a clear shortcoming and will be corrected in the revised manuscript.

"Page 16610, line 9: Area weighted average: the area of each grid box divides the ozone values here?

Answer: Area weighting in this context refers to a spatial weighting to compensate for the unequal geographical area that is represented in gridded data sets on spheric coordinates. In atmospheric science this is commonly simply referred to as "area weighting".

ACPD manuscript, page 16610, line 9ff:
This can also be seen from the daily time-series of polar cap ozone (i.e. area weighted averaged over latitudes ≥50° N; Fig. 2a and b), which closely follow each other in these two years.

"Page 16610, line 22: I do not see thirty years of data here. It is only for the period 1995—2011."

Answer: We agree to the referee. It was poorly formulated and will be corrected in the revised manuscript. Our intension was to refer to the period since the beginning of globally monitoring ozone with satellites, but is meaningless in this context.

"Page 16611, lines 7—9: If you would like to discuss the large variability of Arctic ozone loss, then there are better examples (instead of 2009/2010) such as winters 1998/1999, 2000/2001, 2001/2002, 2003/2004, and 2005/2006. Perhaps, you wanted to discuss the most recent winters?"

Answer: We agree. But we like to point out that for comparison we need to chose a winter from the period when SCIAMACHY operated between 2002 and 2012. Ozone losses in the winters before 2009/10 have been addressed by Sonkaew et al. (2013), so that we decided to take 2009/10 exemplarily for a "warm winter" situation of the Arctic stratosphere, and also with the aim to show new data. In 2009/10, the ozone level was large but also significant denitrification was observed. We revise the paragraph and make clear why we chose this particular period.

"Page 16615, lines 19—24: This has already been demonstrated for a range of Arctic winters in Kuttippurath et al. (2010)."

Answer: We agree and we will cite this study in the revised manuscript.

"Page 16616, para1: Compare your ozone loss estimates with other published results for this winter (2010/2011). Your comparison is not complete here, as there are also other published studies for this winter.

Answer: We agree. During time of manuscript preparation other studies have been published. However, we did not noticed all, and thank the referee for her/his suggestions. In particular, we will cite Kuttippurath et al. (2012) and Strahan et al. (2013), inferring the 2011 ozone losses from MLS, OMI and CTMs. We will also consider the studies of Balis et al. (2011) (that was cited already but not concerning loss estimates) and Isaksen et al. (2012), although both do not show vertically-resolved ozone observations, which are in our focus.

"Page 16620, line 4: What is the reason to compare it only to this particular study here? This is not mentioned/justified here. Please note that there are many published works dealing with modeling of ozone loss for the winters 2005 and 2011."

Answer: The referee is referring to our comparison of modelled ozone losses to those shown by Singleton et al. (2007) for a different winter. Before we comment on that, let us mention that we agree to the referee's opinion, that there are other studies available showing ozone losses from CTMs for the two years she/he mentioned. With respect to 2011 losses, we will consider the missing publications. Partly they were cited already, but related to other aspects of our work.

In Section "3.5.2 Modelled ozone losses", we will additionally cite the studies of Sinnhuber et al. (2011), Kuttippurath et al. (2012), Isaksen et al. (2012), Strahan et al. (2013), and Brakebusch et al. (2013). Please, see also our comments made above referring to your point "*Page 16616, para1: Compare your ozone loss estimates with other published results ... 2011). Your comparison is not complete here...* "

Referring to our comparison to Singleton et al. (2007): We discuss this study in more detail because - to our knowledge - this is the only study that describes a similar behaviour in modelling Arctic ozone during a strong depletion period (in their case winter-spring 2004/2005), that is an overestimation of model ozone losses compared to losses inferred from satellite observations. In their study, a CTM very similar to ours was used (SLIMCAT), that was also set up similarly to our simulations (initial/boundary conditions, meteorology, tracer advection, chemistry). In the revised manuscript we will further emphasise the importance of the study for our work.

"Page 16620, lines 8-14: What about the dynamics/radiation scheme in the model? You mean the chlorine activation in the model is not comparable to measurements? A recent study by Wohltmann et al. (2013) state that the maximum difference of simulated ozone loss from various scenarios, including the differences in PSC and denitrification schemes, is about 10%. It is worth mentioning here if the differences are also within the same range. "

Answer: This comment refers to Section 3.5.2 about the model calculated ozone losses.

The referee's question about the influence of dynamics / radiation on the CTM calculated ozone losses is a very important issue that was not appropriately discussed in the manuscript so far and will be considered in the revised manuscript. We will further emphasise differences in the underlying assumptions of the two methods used in our study to quantify chemically-induced ozone losses from model and limb measurements. Both methods are systematically different in their treatment of radiation and (atmospheric) dynamics, hence, very likely accounting for most of the difference in shown ozone losses. A detailed quantification of individual contributions, however, should be done in a separate study because it would go far beyond the scope of this paper. We agree to the referee that the statement about the impact of the modelled chlorine activation on the spring-time ozone loss was overemphasised. We will rephrase accordingly.

Wohltmann et al. (2013) is an important study to better understand heterogeneous processes that determine polar ozone levels. However, the study has been published at the end of manuscript preparation, and we did not noticed it. In the revised manuscript we will, of course, refer to this work and discuss their findings.

"Page 16620, lines 15-18: Does that mean model dynamics has no role if the LINOZ is used in the model? "

Answer: In the CTM, the LINOZ ozone tracer is advected similarly as (and simultaneously to) the chemically fully active ozone tracer. The latter is used to calculate heating rates with the MIDRAD radiation scheme which in turn are used to calculate the vertical transport of tracers across isentropes. Details are given in Sinnhuber et al. (2003). That means, on synoptic to planetary scales model dynamics do not account for potential differences between the two ozone tracers, although differences between their horizontal and vertical distributions may arise from the different treatments of homogeneous chemistry for the two ozone tracers (fully interactive and linearised chemistry). But differences in the distribution of LINOZ relative to fully interactive ozone do not feed back into the advection of LINOZ.

Over the poles, where during severe winters significant ozone depletion can occur, the diabatic descent of LINOZ may be overestimated (because it is determined by the fully interactive ozone tracer that undergoes ozone destruction). But since chemically-induced ozone losses occur on 4 to 5 model layers only (that is less than a fifth of all model levels), it is expected not to induce a perceptible difference in the ozone loss calculation.

"*Page 16620, lines 19-21: How does the chemistry affect your tracer simulations?*"

Answer:
We are not sure, what the referee is referring to in her/his question. Of course, chemistry is affecting the modelled tracer transport. For instance, stratospheric ozone is more accurately modelled by the fully interactive chemistry setup as we describe here or in the works of Aschmann et al (2009) and Aschmann et al. (2011). The CTM has also capabilities to model stratospheric ozone by means of a linearised ozone chemistry scheme (LINOZ; McLinden et al., 2000; Hsu and Prather, 2009). Although LINOZ has been successfully applied to examine the interannual variability of polar ozone (e.g. Kiesewetter et al., 2010), differences between ozone levels calculated by LINOZ and the fully interactive set up may be large, in particular when chemically-driven ozone depletion is attempted to be modelled. We hope this answer satisfies your concern.

"*Page 16621: lines 13—14: Yes, due to the major warming*."

Answer: Yes, this is correct and will be considered in the respective section.

"*Page 16623, line 1: Put "e.g." before the citations*."

Answer: Your suggestion has been considered.

"*Page 16623, line 6: There is only one final warming, not "a" final warming, but "the final warming"*."

Answer: Yes, this is correct. We correct respective phrase.

"*Page 16623, line 20: Not in "some winters", but in cold winters*."

Answer: Correct, will be revised.

"*Page 16623, line 22: "end of the observations"? State the precise time period*. "

Answer: We are adding the respective date (16 March 2011) at the end of the sentence.

"*Page 16625, lines 20—21: Arctic ozone hole? I thought it showed only signs of an ozone hole, not a well-defined one as in the case of the Antarctic ozone hole*. "

Answer: We agree to the referee and will rewrite the sentence.

"*Page 16625, line 23: Hurwitz et al. (2011) discussed dynamics, not chemical ozone loss. Also, there are some other studies that discussed ozone loss in 2010/2011*. "

Answer: Both is correct. Hurwitz et al. (2011) do not discuss chemical processes. And there are other studies on the subject. We like to state the following with respect to this comment:
- To our knowledge, Hurwitz et al. (2011) was the very first peer review article describing observations of unusually low ozone over the Arctic in winter-spring 2011. It was published before Manney et al. (2011) which focussed more on the chemical perspective.

- The sentence that is criticised here belongs to the preface of our ozone mini-hole examination, and is not investigating the chemically-induced ozone losses later in that year. The criticised sentence cites some studies that were made before our study was published, studies which were accepted during time of manuscript preparation. Between submission of our manuscript and the current review a few other studies have been published, that, of course, will be considered in the revised manuscript. We apologise, when we missed the one or another publication which is worth to be cited here or elsewhere in our manuscript.
- In the criticised sentence it is clearly stated that the cited studies addressed not the ozone mini-hole condition, instead they addressed causes and strength of the chemically-induced losses. That means, they refer to the broader scientific context. This includes also Hurwitz et al. (2011), in our perspective, because this study examines causes for the ozone losses in spring 2011. From a dynamical perspective, but that is also a valid approach.
- And all the studies cited here, incl. Hurwitz et al. (2011) and the others, do not mention or discuss low ozone in 2011 before chemical processing destroyed most of it in spring.

Therefore, we think it is appropriate to cite Hurwitz et al. (2011) in this section.

"Page 16633, last paragraph before conclusions: These statements are too general. Please write something from your study here. "

Answer: We will follow the referee's suggestion and rewrite the paragraph. Our attempt here was to provide a short summary of the analysed relationships between the ozone mini-hole and the large ozone losses later in 2011, and to indicate how important such situations might be when the climate changes, quasi as a kind of outlook or stimulus for further research on the subject. It might be better to state these more general things in the conclusions.

"Page 16634, lines 18—20: Since you haven't shown this (contribution of the chemical cycle to the ozone loss) in this manuscript, you need to cite appropriate references here. "

Answer: In our opinion we showed in Section 3.3 (limb vortex-averages) both the differences in the variability in the two four-month periods that is larger at the upper isentropes in 2010 than in 2011 and we showed that in 2010 above 550K NOx depletes approximately two times more ozone than in the layers below, where mainly heterogenous processes are responsible for ozone depletion:

- "... Differences in the vortex dynamics in the two years explain the obvious differences seen in the ozone time-series above 550 K: in 2010, when the vortex was much weaker than in 2011, the variability in the ozone profiles is quite large in the upper layers. Higher temperatures in a weaker vortex 2010 go along with a higher variability in the descent of air from above (descent is stronger in weaker vortices; Rosenfield et al., 1994), contributing to the variability at the ozone mixing ratio maximum within the vortex....." (page 16613, line 9ff)
- "...By comparison, in the warmer and weaker Arctic vortex 2010 ozone losses barely exceeded 20% below 550 K. Above the 550 K isentropic surface, however, we infer an ozone depletion of up to 40% during spring 2010 ... Above the regions where halogen driven catalytic cycles remove ozone, NOx ... photochemistry is predominantly responsible for ozone depletion (Osterman et al., 1997). This process is stronger during warm winter years when the vortex is weaker ..." (page 16615, line 13ff)

However, we will follow the referee's suggestion and include references to other studies here.

Comments on figures

"Figure 2: What lines are min, mean, and max? Please make a good figure legend here to differentiate the lines."

Answer: We rephrased the figure caption, see below.

ACPD, Fig.2 caption:

Evolution of Arctic total ozone in the GSG data set for various cold winters with severe ozone losses since 1995. (a) shows the area weighted mean and (b) the minimum total ozone as obtained from the GSG data set north of 50°N. Each time-series is also confined by respective mean, minimum and maximum values of the 16-year data record 1995 - 2010.

Excerpt from revised manuscript, Fig.2 caption:

Fig. 2. Evolution of Arctic total ozone in the GSG data set for various cold winters with severe ozone losses since 1995. (a) area weighted mean and (b) the minimum total ozone as obtained from the GSG data set north of 50°N. Coloured lines show time-series of individual Arctic winters as indicated. The solid black line is the mean of all years in the 16 year data record (1995-2010). The dotted line shows the maximum and minimum value observed during the entire data record.

"*Figure 3: I do not find any reason to include this figure, as it gives no additional information. If you are discussing dynamics, you could refer Hurwitz et al. (2011) and Kuttippurath et al. (2012).*

> *Kuttippurath, J., Godin-Beekmann, S., Lefèvre, F., Nikulin, G., Santee, M. L., and Froidevaux, L.: Record- breaking ozone loss in the Arctic winter 2010/2011: comparison with 1996/1997, Atmos. Chem. hys., 12, 7073-7085, doi:10.5194/acp-12-7073-2012, 2012.*"

Answer: Please see comments made above ("*Since this is a scientific manuscript .. and by removing the figures 3 and 4. Describe only ... figures 3 and 4 do not give any additional information about the... Hurwitz et al. (2011) and Kuttippurath et al. (2012) ... You need to cite these publications here*") with respect to this figure. We will carefully revise this suggestion.

"*Figure 5: There was no well-defined vortex from March onwards in 2009/2010 (due to the major warming in early February). So you need to cut the data afterwards in March.*"

Answer: We understand your comment. Arithmetically, in March and April 2010 a condition can be found in the Arctic stratosphere which fulfils the vortex criteria by Nash et al. (1996) (see also our comments to the point your raise "*Page 16608, line 11: Any reason for selecting this particular value as vortex edge?*"). The vortex was disrupted by a final warming in early February 2010, that is correct. On the other hand, PV distributions at 475 K over the Arctic indicate that after 25 February 2010 the situation stabilised and is traceable until 9 April 2010. Whether this synoptic situation can be seen as a vortex or not, it fulfils our vortex boundary criteria of 38 PVU according to ECMWF reanalysis. But, of course, the area enclosed by 38 PVU is significantly smaller as in the month before the warming and also smaller than in the same period in 2011. PV values in e.g. March 2010 were clearly above 50 PVU, which is still more than 2/3 of average PV within the vortex in March 2011.

We will rework all respective figures and indicate the February 2010 final warming to make clear that the state of the polar stratosphere is different from that in 2011.

"*Figure 6: Please put the same scale/colorbar for comparison with Figures 5 and 8.*"

Answer: We have tested this when the manuscript was prepared. It turned out that then a lot of information will be lost due to the different mixing ratio levels between the limb observations and the solar occultation measurements. When the contouring interval of Fig. 6 is used also for the other two figures, any variability in limb and CTM vortex-averages is not seen any more. In Section 3.4 we describe in detail why the mixing ratio levels are so different (solar occultation profiles are not representative vortex conditions, sampling too sparse).

"*Figure 7: All dots are of the same size in these plots!*"

Answer: We rework Fig. 7 in order to enhance its quality. Following changes have been made:
- Now black is used to mark locations of limb profiles within the vortex (before we used grey shades that conflicted too much with the colour coding of the PV distribution).
- Now we use a different symbol to mark solar occultation measurements in order to better separate them from the limb profiles.
- We are also introducing colours to distinguish solar occultation measurements within and outside the vortex.

"*Figure 8: Please see the comment for Figure 5, and also use the same colorbar/scale for each plot as in Figure 5*."

Answer: Please see respective comments above.

"*Figures 9, 10: Select the winters you would like to discuss and show plots for those winters only. It is slightly disturbing to have different winters for different plots*."

Answer: We will rework the two figures and adjust all parameters, colours etc. We will also critically examine whether we consider only these winters which are most important for our discussion.

Technical corrections:

"*Page 16599, line 13: Delete "see for example" and then write "e.g."* "
Answer: We will follow this suggestion.

"*Page 16608, line 12: Full stop after 80 N*. "
Answer: Typo, will be changed.

"*Page 16608, line 16: Better to use "similar conditions" instead of "equal conditions"*. "
Answer: We will follow this suggestion.

"*Page 16608, line 23: Delete "slightly" (5—15% is mentioned and is not a "slight" increase)*. "
Answer: This is correct and will be changed.

"*Page 16609, line 8: Sander et al. 2006, not JPL/NASA*. "
Answer: This is correct and will be changed.

"*Page 16609, line 16: "start in"* "
Answer: Typo, will be corrected.

"*Page 16609, line 17: "to estimate the ozone loss"* "
Answer: We will consider this suggestion.

"*Page 16614, line 6: Please re-write the sentence*. "
Answer: We will follow this suggestion and rewrite the sentence.

"*Page 16614, line 21: "a strong vortex less air from"?* "
Answer: We revise this paragraph.

"*Page 16618, line 9: "per second larger"* "
Answer: We meant "per se", synonymous to "simply", "in general". We will rephrase and make it clear.

"*Page 16620, line 2: “slight underestimation”* "
Answer: Typo, will be corrected.

"*Page 16620, lines 2-3: “modeled ozone loss” or “simulated ozone loss”, not “modeled induced ozone loss”.* "
Answer: We will follow the referee's suggestion and rework respective phrase.

"*Page 16621: lines 10-11: “total supply of PSCs”?* "
Answer: We will rework the entire paragraph.

"*Page 16622, lines 10, 16: “information” not “informations”* "
Answer: Typo, will be corrected.

"*Page 16623, line 1: (e.g. Manney et al., 2011, …) as there are many analyses available for this winter.* "
Answer: *We will follow the referee's suggestion and change it accordingly.*

"*Page 16625, lines 13—14, Use “prolonged” or a similar word, not “long-lasting”.* "
Answer: We will follow the referee's suggestion and rework respective phrase.

"*Page 16626, line 8, “dissipated” is better than “dissolved”* "
Answer: We will follow the referee's suggestion and rework respective phrase.

"*Page 16626, line 9: Delete “the” before 6 February.* "
Answer: We will follow the referee's suggestion and rework respective phrase.

"*Page 16629, line 1: vortex “slides”?* "
Answer: Typo, will be corrected.

"*Page 16630, line 6: “it” did?* "
Answer: Typo, will be corrected.

"*Page 16630, line 12: “onwards”* "
Answer: Typo, will be corrected.

"*Page 16630, line 16”: “never slide”* "
Answer: Typo, as above, will be corrected.

"*Page 16632, lines 15, 19: delete “perfectly”* "
Answer: We will follow the referee's suggestion and rework respective phrase.

"*Page 16645, line 26: the year should be 2011.* "
Answer: This is correct and will be changed.

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