We would like to thank reviewer #2 for his/her corrections and recommendations. Additions to the text are highlighted in blue and text that has been removed from the original text is highlighted in red. The reviewer comments are included in bold.

General remarks

Let me start by saying that this is a very good paper. It has many strengths and using a high quality data set such as ACE-FTS to perform a consistent analysis of Arctic ozone loss constitutes a very valuable scientific study. Using six different methods (and comparing the results) and employing the information from a 'simulation only' analysis is an achievement.

Nonetheless, I have reservations about the paper which concern the conclusions drawn from the paper and the discussion of the different methods. I also think that that the existing literature on the subject should be discussed in a more balanced way. Further, some additional references (of course not necessarily every single paper mentioned in this review) should be taken into account.

My first general point is that I suggest improving the discussion of the various methods. In the paper it is stated that the tracer-tracer method "neglects descent from high altitudes". This is a bit vague, what means "high altitudes" in this case? Perhaps "above the ozone maximum", where ozone is no longer chemically inert? This should be clarified. And there are a number of studies that address the point of descent from high altitudes in the tracer-tracer method (see below) that should not be neglected. The same is true for the issue of "mixing across the vortex edge" which is discussed for the tracer-tracer method. The basis for the discussion in the manuscript so far is the important work by Michelsen et al. (1998) and Plumb et al. (2000), but there is also a discussion of the arguments presented in these papers in the literature which should not be neglected (e.g., Mller et al., 2005, cited in the manuscript, but not in this context) and (Salawitch et al., 2002, not discussed in the manuscript so far, see also below).

Further, it seems to be tacitly assumed in the manuscript that these issues (high altitude descent and mixing across the edge) do not affect the other methods used in this study. (See for example the discussion on the artificial tracer method below). I strongly suggest to describe the impact of these issues on all considered methods.

Perhaps most importantly, if I accept the conclusion from the paper that the descent method is the most reliable one (at least it seems to be a method with very small error estimates), there appear to be significant differences between the SLIMCAT simulations (SLIMCAT only) and the estimated ozone loss. In the sense that SLIMCAT overestimates the chemical ozone loss. For example for 2009/2010 the

simulated loss is 51 - 7 DU and the descent deduced loss is 13 ±3 DU, for the winter 2004/2005, the comparison is 67 ± 3 and 47 ± 4 DU. (Are these error estimates for ozone loss really comparable). Perhaps I am misreading this conclusion on the reliability of the methods, but the manuscript should provide a clear guidance to what the message of the paper is in this respect. Thus, I think the paper should discuss these questions in more detail and make a clearer statement on simulated versus observed ozone loss. In summary, I suggest a more extensive and a more balanced discussion of both the employed methods and the obtained results in a revised version of the manuscript.

We thank the reviewer for the thorough review and great suggestions to improve our paper. These general comments have been addressed and detailed comments are provided in the revisions below.

Artificial tracer method
The authors use the artificial tracer method by Esler and Waugh (2002), who have developed the method for NO_y in midlatitudes: they report that they construct an "artificial 'reference tracer' from a linear combination of other long-lived tracers. The reference tracer is designed so that, as far as possible, it has a linear canonical relationship with NO_y in midlatitudes".
The further development of the technique for analysing ozone loss was done by Jin et al. (2006). They state: "The decrease of O_3 with respect to this artificial long-lived tracer can be regarded as the chemical O_3 loss. However, because the correlations inside and outside the vortex are different, this method cannot correct for the mixing across the vortex edge. This kind of mixing can only increase O_3 for an artificial tracer value, which suggests that neglecting mixing across the edge gives a conservative O_3 loss estimate for this method". This concept is also illustrated in Fig. 1 of this review.
Thus it is an oversimplification to say that the artificial tracer method compensates for 'mixing'. If mixing occurs across the vortex edge, this will be mixing between two different ozone-tracer relations, even though both could be linear (Fig. 1). And then mixing would have an effect in tracer-tracer space. The authors might not agree with this concept, but I think a discussion is necessary.

We have included further discussion of the artificial tracer method, correcting the statement that the artificial tracer correlation method corrects for mixing across the vortex edge. The first paragraph of this Sect. 3.2 has been changed according to the suggestions provided by the reviewer:

"The amount of mixing of extra-vortex air into the polar vortex varies widely depending on the dynamics of each winter and spring, and is more likely to occur in the NH (WMO, 2014). Neglecting mixing processes from the edge of

the polar vortex ~~could result in an overestimation of the chemical ozone loss when using the tracer-tracer method, and mixing within the vortex from high altitudes~~ or mixing of high altitude air (above the ozone maximum) can lead to an underestimation of the chemical ozone loss (e.g., Rex et al., 2002; Müller et al., 2005). One method that provides a ~~mixing correction , in addition to~~ correction for both mixing from the vortex edge and for descent, is the artificial tracer method. This method was first proposed by Esler and Waugh (2002) and uses a "tracer" created from a linear combination of several different trace gases that is linearly correlated with ozone. This linear correlation makes it easier to determine the ozone loss and reduces the impact of mixing, since mixing from the edge of the vortex would only result in "moving" the air parcels along this linear correlation line (Esler and Waugh,2002). Initially such an artificial tracer method was used by Esler and Waugh (2002) to estimate denitrification inside the Arctic polar vortex. However, this same method can be applied to estimating the chemical ozone loss as was done by Jin et al. (2006). While it reduces the error from mixing of air near the vortex edge, this method, however, does not account for mixing of extra-polar vortex air into the vortex. The artificial tracer, established from observations inside the polar vortex does not follow the same linear correlation outside the polar vortex (Jin et al., 2006).

We also changed the fourth paragraph in Sect. 4.2, p.16, l.15:

"Discrepancies are apparent between the measurement only methods and the passive subtraction ~~methods in~~ using CTMs for 2010, especially for the computed mean partial column loss. Each time the vortex splits and the two parts reunite, extra-vortex air is mixed. In 2010 the polar vortex was very disturbed, therefore, ~~for 2010, the~~ methods that do not account for the mixing of extra-vortex air (the tracer-tracer ~~and~~ method, the profile descent techniques and the artificial tracer technique) are not reliable for that year since an isolated vortex is essential for these methods~~that do not account for the mixing of extra-vortex air. The results of the artificial tracer technique should be uninfluenced by mixing. The~~ . The loss estimates in 2010 using the ~~artificial tracer technique~~ measurement only techniques do not agree with the passive subtraction ~~methods. It is worth noting that the passive subtraction methods compute similar losses from year to year, including~~ using CTMs. Generally, we see the largest differences between the passive subtraction method using CTMs and methods that use measurements only for years with strong turbulence and relatively small ozone loss (see Table 1).For example in 2010, ~~when the vortex was much disturbed. The~~ the passive subtraction methods ~~may smooth out the year-to-year differences and model results in some years may compute some ozone loss even in the absence of chemistry~~using CTMs are nearly twice as high for the maximum ozone loss and more than three times as high for the mean ozone loss than the methods that use measurements only. This could either be due to mixing processes unaccounted for in the methods using measurements only or the passive subtraction methods using CTMs may overestimate passive ozone."

Additionally, we have changed our wording in the final conclusions and discussion:

" ~~This analysis shows~~ Based on this study, for years with a stable and strong polar vortex, the tracer-tracer technique, the artificial tracer technique and the passive subtraction using both CTMs lead to similar ozone losses and seem to estimate a similar passive ozone profile. We also found that from the six different estimation methods presented, ~~either~~ the artificial tracer correlation technique ~~or~~ and the passive subtraction method (with ATLAS or SLIMCAT) ~~is~~ are best suited for estimating the ozone loss in the Arctic polar vortex. ~~Based on this study, for years with significant activation either~~ For years with an unstable polar vortex we recommend using the passive subtraction ~~or the artificial tracer technique are best suited. For years with little to no activation~~ technique, since the artificial tracer ~~correlation technique might be the most reliable because it considers mixing and seems to compute a reasonably small ozone loss~~ technique does not account for mixing of extra-polar vortex air. We did not find any difference between an Eulerian or a Lagrangian model and found that both types of CTMs seem to compute the Arctic ozone loss equally well. . "

Tracer-tracer method

In the manuscript it is stated that the tracer-tracer method (e.g., p. 7, l. 11) "neglects descent from high altitudes". This is a bit vague, what means "high altitudes" in this case? Perhaps "above the ozone maximum", where ozone is no longer chemically inert? However, e.g. Salawitch et al. (2002) (in a study using tracer-tracer relations) consider data up to 8.9 hPa, which is a relatively high altitude in the stratosphere. They also discuss the question of ozone at higher altitudes (Salawitch et al., 2002, see paragraphs [43] and [44]).

In particular, Salawitch et al. (2002) state that the "*Plumb et al. [2000]* model results for χ_2 versus χ_1 are driven primarily by supply of air at the top of the vortex with near zero mixing ratios of both species. Our observations exhibit a critical difference with respect to these heuristic model calculations. The OMS measurements show that the top of the Arctic vortex is supplied with air having mixing ratios of O_3 between 3 and 4 ppm, considerably higher than the final value of $[O_3]$ in the inner vortex". Moreover, there could be intrusions of mesospheric air to lower altitudes, which are discussed by Müller et al. (2007), in a case study. They conclude that "measurements influenced by mesospheric air show ozone mixing ratios ranging between 3.6 and 5.6 ppm, which are clearly greater than those found in the "early vortex" reference relation employed to deduce chemical ozone loss".

Rex et al. (2002) is cited (p. 7., l. 8) in support of the criticism of the tracer-tracer method. And indeed, Rex et al. (2002) mention that "*Michelsen et al. [1998]* and *Plumb et al. [2000]* have suggested that

before chemical loss of ozone occurred, mixing between subsided inner vortex air with extravortex air may lead to a flattening out of the curved O_3/N_2O relation and thus may be mistaken as chemical loss of ozone. However, ... " But the overall conclusion of Rex et al. (2002) on this issue reads: "Thus the overall changes in the O_3 versus N_2O relation observed during the course of winter could not have been caused by transport, and rather represent a lower limit for the true chemical loss of ozone". Therefore, I suggest rethinking of how to use this citation in the paper.
Further, especially for the winter of 1999/2000 there are a number of studies (not taken into account in the manuscript so far) that argue that transport alone could not have led to the observed changes in the O3 versus N2O relation (tracer-tracer method).
Richard et al. (2001) report that "there is relatively little change in the ER-2 O_3:N_2O [...] relationships over the two week period between 20 January and 3 February 2000. Additionally, the O_3:N_2O profiles are found to be similar to the early winter vortex balloon profiles which allow extension of the relationships to regions above the ER-2 flight altitudes thus defining the chemical composition of air that later descends to ER-2 sampling altitudes (18-21 km). [...] Therefore, these relationships allow for the establishment of a winter vortex reference to quantify O3 chemical loss occurring during late February/early March2000."
Another piece of evidence on these issues is provided by Ray et al. (2002) who find that "mixing of midlatitude air into the winter vortex is not a significant contributor to the observed ozone changes in the 1999/2000 season".
Of course, the authors do not need to follow/accept these arguments but I think a more balanced discussion in the manuscript is necessary rather than relying mainly on the arguments of Michelsen et al. (1998) and Plumb et al. (2000) here.

We have re-written some of the text in the paper and included a more balanced discussion of the tracer-tracer correlation method as suggested by the reviewer, and included the references as suggested.
We have changed the first paragraph of Sect. 3.1 according to the reviewer's suggestions:

"As described in Sect. 2.2, measurements taken in January inside the polar vortex are used to quantify the ozone distribution before significant ozone depletion occurs. This dataset is then compared to measurements taken in March, when chemical ozone depletion is most pronounced in the observed ozone profile. This method has been criticized for neglecting processes that mix extra-vortex air into the polar vortex ~~(e.g., Rex et al.,2002)~~ (e.g., Michelsen et al., 1998b; Plumb et al., 2000, 2003; Plumb, 2007), because it assumes that the polar vortex is isolated, which is not true for all years, especially in the Arctic. ~~By~~ On the

other hand, some studies observing Arctic ozone loss the 1999/2000 winter (a winter with an unusually strong polar vortex and thus little mixing) have found that mixing of mid-latitude air was not a significant contributor to the observed changes (e.g., Richard et al., 2001; Ray et al., 2002). In our study, using the sPV criteria described above, we attempt to limit the influence of mixing of extra-vortex air in our calculation of the early vortex reference function.

The tracer-tracer correlation method also neglects descent of ozone or the tracer from high altitudes ~~that invalidates the use of~~ (middle and upper stratosphere and mesosphere) above 550 K that is not included in our calculation of the early vortex reference function. However, Salawitch et al. (2002) showed that supply of ozone depleted air into the top of the vortex did not play a role in the subsequent evolution of the ozone-tracer relation in the 1999/2000 Arctic winter (where the vortex was strong). Mixing of air from top of the Arctic vortex (where mixing ratios are between 3 and 4 ppm (Salawitch et al., 2002)) into the polar vortex, could, however, underestimate the ozone loss of the tracer-tracer ~~relationships that include only lower to middle stratospheric data (e.g., Michelsen et al., 1998 a, b, 2000; Plumb et al., 2000, 2003; Plumb, 2007). Consequently, this could result in a different profile of ozone loss for each tracer~~method. Rex et al. (2002) state that the tracer-tracer correlation represents a lower limit of the true ozone loss in the case of the 1999/2000 Arctic winter (a year with a stable polar vortex). "

And we included changes in Sect. 4.2, p.16, l.1:

"In some years, the tracer-tracer correlation method and the average vortex descent technique differ significantly from all other estimation methods. These ~~discrepancies highlight the difficulties~~ differences highlight the difficulty of using the tracer-tracer correlation method, because mixing processes and descent in the 2005 Arctic vortex are not ~~considered, and the difficulties~~ accounted for. These differences also highlight the difficulty of using the average vortex descent technique in years of an unstable polar vortex. ~~This can therefore lead to an overestimated ozone loss using the tracer-tracer correlation method and an underestimated ozone loss using the average~~ The average polar vortex descent technique ~~. In all other years, the tracer-tracer correlation method agrees well with the other five methods. The average vortex descent technique~~ typically underestimates the ozone loss compared to all other methods, this technique only agrees well with the other methods in March 2007 ~~,~~ and 2008."

Comments

p. 2., l. 7: "Here we show" I think it is not really new that these tracers are suitable.

We changed the sentence to:

"For the tracer-tracer, the artificial tracer, and the average vortex profile descent

approaches, various tracers have been used ~~. Here, we show~~ that are measured by ACE-FTS. From these seven tracers investigated, we found that CH_4, N_2O, HF, and CFC-12 are the most suitable tracers for investigating polar stratospheric ozone depletion with ACE-FTS."

p.2, l. 20: This is true for the time period of elevated stratospheric chlorine and bromine.

We have changed the sentence to:

"Arctic ozone column loss is extremely variable in the winter/springtime and can range from near zero to about 150 DU..."

p. 3., l. 1: Suggest citing here also the early theoretical study by Carslaw et al. (1994).

The citation has been included.

p. 3., l. 1, 2: Perhaps helpful: information on observed PSCs is now also available from MIPAS (Spang et al., 2017).

The citation has been included.

P. 3., l. 6: The paper by Solomon et al. (1986) is mostly about heterogeneous chlorine activation, less on the relevant catalytic ozone loss cycles.

The citation has been removed, only McElroy et al. (1986) and Molina and Molina (1987) are cited in this sentence now.

p. 3., l. 8: the point is that low temperatures are required but they need to last long enough into the period when sufficient sunlight is available to drive the ozone loss (as it is the case regularly in the Antarctic).

This sentence has been added to clarify:

"For polar ozone loss, low temperatures are required but they also need to last long enough into the period when sufficient sunlight is available to drive the ozone loss.

p.3., l. 28: This statement is also true for the Antarctic. Perhaps look at ozone loss estimates for the Antarctic.

We wanted to highlight that the dynamic variability is stronger in the Arctic, and SSW occur more frequently in the Northern Hemisphere. The sentence

has been rephrased to:

"Because of the strong dynamical variability of the Arctic polar vortex, quantifying chemical ozone loss is more challenging in the Arctic~~is challenging~~."

p. 4., l. 2: I do not think this statement is correct as written here, see the detailed discussion.

The sentence has been changed to:

"Using an artificial tracer(e.g., Esler and Waugh, 2002; Jin et al., 2006) that is constructed (from observed trace gases) to be linearly correlated with ozone can improve the accuracy of the loss estimate~~, since that linear relationship will not be changed by mixing processes (see Esler and Waugh (2002) for more details)~~."

p. 7., l. 8: Rex et al. (2002) is cited here in support of the criticism of the tracer-tracer method. However Rex et al. (2002) state that "Thus the overall changes in the O_3 versus N_2O relation observed during the course of winter could not have been caused by transport, and rather represent a lower limit for the true chemical loss of ozone". I think some of the citations used in l. 12 of this page are more appropriate here.

As described above (in the discussion of the artificial tracer technique), we have included these suggested changes in this paragraph.

p.7., l. 12: I cannot see what the contribution of the citation to Michelsen et al. (1998, GRL) is here. This paper does not discuss ozone. Either explain why the citation is needed or drop the citation.

Michelsen et al. (1998,GRL) is a critical reference showing observational evidence for different mixing lines inside and outside the vortex and at different times within the vortex, and as such is a foundation for any use of tracer correlation methods for ozone loss.

p. 7., l. 15: add 'over a polar season'; of course tracers like methane or CFC- 12 *are* influenced by chemical processes, otherwise there would be no vertical profile.

This has been added as suggested.

"A tracer is required to be long-lived and stable (Plumb and Ko, 1992) and thus, not influenced by chemical processes over a polar season."

p. 7., l. 28: "neglecting mixing processes from the edge [...] over estimation of chemical ozone loss... ". I do not think that the papers

cited here make this point. See statement from Rex et al. (2002) above. Also, Müller et al. (2005) state in the abstract that "mixing across the polar vortex edge impacts ozone-tracer relations in a way that may solely lead to an 'underestimation' of chemical ozone loss and not to an overestimation"'; this discussion needs to be revised.

We have revised this discussion accordingly, as stated above (in the discussion of the tracer-tracer technique).

p. 10., l. 9: If there is descent from higher altitudes, as discussed in the paper elsewhere; would the 'passive ozone assumption' hold? If ozone is not in complete darkness, it is not passive at higher altitudes.

In this study we focus on O_3 between approximately 380 to 550 K. For this altitude (the lower stratosphere), the time-scale for dynamical changes is much shorter that that for chemical changes (unless there is heterogeneous PSC-mediated chemistry). The region where chemical and dynamical time-scales are similar is around 30 to 40 km (800-1000K). The region studied in this paper is well below that altitude, and below the region where gas-phase chemistry would affect the passive ozone in situ. If ozone was transported down from 700 or 800K to the 350-550 K region, in its new environment any chemical reactions would take place at the rates consistent with that altitude, and thus would be very slow compared to those for transport. So any significant affect on the passive ozone should be negligible at and below 550 K. E.g., Singleton et al. (2005) compared two model runs with the gas phase chemistry turned on and the other one off, the differences increased with altitude, but remained below 0.5 ppmv for 550 K and are negligible for lower altitudes in March.

Singleton, C. S., Randall, C. E., Chipperfield, M. P., Davies, S., Feng, W., Bevilacqua, R. M., Hoppel, K. W., Fromm, M. D., Manney, G. L., and Harvey, V. L.: 2002-2003 Arctic ozone loss deduced from POAM III satellite observations and the SLIMCAT chemical transport model, Atmos. Chem. Phys., 5, 597-609, https://doi.org/10.5194/acp-5-597-2005, 2005.

p.10., l. 18: what about Wohltmann et al. (2017) here?

We have included the citation.

p. 10., l. 32: Why HISPLIT? Would it not be more consistent to calculate the trajectories with the (diabatic) trajectory scheme of ATLAS. And likewise not change the meteorological analysis?

For practical purposes the trajectories were estimated with HYSPLIT. We agree that possibly it would have been more consistent to calculate trajectories with the diabatic trajectory scheme of ATLAS, however, the changing this would require and effort that is not justified by the benefit. Small changes of the tra-

jectories will have a negligible effect on the end results.

p. 12., l. 1: Is there a comparison (or comments along this line) between the polar chemistry of SLIMCAT and ATLAS?

In this study, we did not use the results from the chemical model of ATLAS, and therefore did not discuss the differences between the chemical ozone scheme of the ALTAS and SLIMCAT model. As such, we did not perform a detailed comparison between SLIMCAT and ATLAS, and there is currently no paper on this subject.

p. 13, l. 14: It is true that it is worrying that tracer profiles do not agree well for March 2005 (which is not shown directly in Fig. 6a). However, I do not understand why this is only a problem for the tracer-tracer method. I suggest that you also discuss the impact of this finding on other ozone loss estimates considered here. Also the column ozone loss estimates do not seem to differ too much (perhaps with the exception of OCS) for the different tracers in 2005 (Fig. 6).

We have removed the last two sentences of this paragraph and included the following sentence on p.13:

"This indicates the ~~failure~~ shortcomings of the tracer-tracer correlation method, even ~~though~~ in case where only inner core vortex measurements were used for estimating the ozone loss. These results are ~~in agreement with the discussions of the tracer-tracer correlation method in~~ consistent with previous studies (e.g., Michelsen et al., 1998 a, b, 2000; Plumb et al., 2000, 2003; Plumb, 2007) that ~~further confirmed the~~ have shown tracer-tracer ~~correlation method to be inaccurate~~ correlations are not expected to be accurate for estimating Arctic ozone loss. However, in this study, though the profile loss estimates are different for different tracers, the partial column losses (maximum and mean) are not significantly different and agree within the estimated uncertainties. "

We have also added a sentence describing the profiles for the other two methods, on p. 14:
"The profile loss estimated for these different tracers looks similar for most years with the exemption of OCS in 2005, 2008 and 2011, and CCl_3F in 2010."

p. 13., l. 23: explain why this is likely.

We have changed the sentence to:

"Also, in 2007, the estimated loss is larger ~~if~~ when HF is used as a tracer, ~~likely because of mixing~~and does not follow the ozone loss profile as estimated with other tracers."

p. 13., l. 33, 34: it seems obvious to me that average decent profiles will have a small standard deviation, but perhaps I misunderstand. In any event it would be good to give a citation for the smaller uncertainties and how they are calculated.

Section 3.2 explains how the uncertainties are estimated for the average vortex profile descent technique and highlights some of the difficulties estimating this uncertainty that might result in underestimating the true uncertainty. The following has been added:

"Note, this method only allows the estimation of one vortex averaged passive ozone profile; all other methods applied in this study estimate a passive ozone mixing ratio for each data point in March. Consequently, this method does not consider any changes of the passive ozone levels that can occur throughout March. The uncertainty of the passive ozone is estimated based on the $\pm 1\sigma$ standard deviation of the average vortex profile descent (that is quite small for the average vortex profile descent technique). To obtain the total uncertainty, the statistical fitting error of the ACE-FTS tracer measurements and the uncertainty of the passive ozone are added in quadrature. This uncertainty estimate is based on statistical errors only and as such underestimates the true uncertainty. It is difficult to estimate the true uncertainty in this case, because of the unknown effect of ozone due to mixing processes."

And we have included the following sentence in Sect. 4.1, p.14, l.12:

"Note that this does not represent the true uncertainty but more of a statistical uncertainty, since there is only one passive ozone profile for each March (and, therefore, the same amount of ozone at each potential temperature level) the uncertainty is likely much higher. "

Additionally, we have included a sentence highlighting some of the difficulties using the average polar vortex descent technique, in last paragraph of Sect. 4.2, p. 17 l. 26:

"Overall, we have found that the different methods agree in most years within the estimated uncertainties considering the profile mixing ratio loss, as well as the mean and maximum partial column ozone loss. Typically, the average vortex profile descent method estimates smaller ozone losses compared to all other methods. This method provides an approximate ozone loss estimate, however, from only one passive ozone profile, and hence, the passive ozone is the same throughout the month at each potential temperature level"

p. 18., l. 24: There also could be more comparison here with results in the literature based on the methods used in this study. For example, Tilmes et al. (2006) and Rösevall et al. (2008) report chemical ozone loss for the Arctic winter 2004/2005.

We have added the following in Sect. 4.2:

"A comparable partial column loss (120 DU) to our loss estimate using the tracer-tracer correlation method has been estimated by Tilmes et al., 2006 with satellite-borne HALOE observations using the tracer-tracer correlation method.The peak ozone loss in 2005 has also been estimated by Rösevall et al. (2008) using the tracer-tracer correlation technique (with the satellite-borne MLS and Sub-Millimetre Radiometer (SMR) instruments) that is around 1 ppmv and more comparable with our other loss estimates. "

p. 2, l. 32: drop 'ice' here this discussion is about NAT and STS

We have changed the sentence accordingly to:

" PSCs that contain primarily ~~ice~~ particles..."

p. 3., l. 13: an Arctic...

This sentence has been modified according to Björn-Martin Sinnhuber's review and this part of the sentence has been removed.

"In January 2012, very strong polar vortex disturbance occurred ~~, likely due to a Arctic Polar-Night Jet Oscillation Event (Berhard et al., 2012; Chandran et al., 2013; Hitchcock et al., 2013)~~ (Berhard et al., 2012; Chandran et al., 2013) ."

p. 5., l. 6: trace gases

This has been fixed.

p. 6., l. 21: I would formulate: ... "not a sufficient number of measurements" ...

The sentence has been changed to:

"... consequently there were not sufficient number of measurements inside the polar vortex in March to perform the analysis with ACE-FTS."

p. 8, 9: Eqs. (1) (4): do not use italics for ppb and ppt

This has been fixed.

p. 9., l. 26: here and elsewhere, use proper minus signs; i.e. -25 rather than -25

We changed the minus signs accordingly throughout the text.

p. 10., l 10: *is* applied

We changed this sentence accordingly.

p. 11., l. 27: citation for SLIMCAT chemistry scheme?

We changed the sentence to:

"It contains a detailed stratospheric chemistry scheme including all processes that are related to polar ozone depletion (Chipperfield et al., 2006; Dhomse et al., 2013; and references therein). ."

p. 19, l. 24: "...ACE, also known as SCISAT" is this really true?

The CSA refers to ACE in this manner.

p. 19., l. 4: citation for loss in 2010? Also same line 'larger' than what?

We have changed the sentence to:

"For a highly disturbed vortex~~and little to no activation (~~, e.g. ~~,~~2010~~)~~, the passive subtraction methods using CTMs indicate larger ozone loss ~~and~~ than the methods that use measurements only, indicating that either measurement only methods underestimate the ozone loss due to unaccounted mixing processes or the the passive subtraction methods using CTMs might smooth out the year-to-year variability by overestimating passive ozone. "

References: there are still a few typos, missing spaces, additional spaces, etc.

We have corrected the following typos and spelling mistakes that we were able to find. Please let us know if there are any more specific typos and spelling errors that we should correct in the list of references.

Bernhard, G., Manney, G., Fioletov, V., Groo, J.-U., Heikkilä, A., Johnsen, B., Koskela, T., Lakkala, K., Müller, R., ~~Lund~~ Myhre, C. L., and Rex, M.: Ozone and UV Radiation, in: State of the Climate 2011, Bull. Amer. Meteor. Soc., 93 (7), S129–S132, 2012.

Fromm, M. D., Bevilacqua, R. M.~~Bevilacqua, J. Hornstein,~~ , Hornstein, J., Shettle, E. P.~~Shettle, K. Hoppel, and~~ , Hoppel, K., and Lumpe, J. D.~~Lumpe~~: An analysis of Polar Ozone and Aerosol Measurement POAM II Arctic stratospheric cloud observations, 1993–1996, J. Geophys. Res., 104, 24,34124,357, 1999.

Goutail, F., Pommereau, J.-P., ~~Lef'evre~~Lefèvre, F., van Roozendael, M., Andersen, S. B., Kåstad Høiskar, B.-A., Dorokhov, V., Kyrö, E., Chipperfield, M. P., and Feng, W.: Early unusual ozone loss during the Arctic winter 2002/2003 compared to other winters, Atmos. Chem. Phys, 5, 665–677, doi:10.5194/acp-5-665-2005, 2005.

Harris, N. R. P., Rex, M., Goutail, F., Knudsen, B. M., Manney, G. L., ~~Müller~~Müller, R. and von der Gathen , P.: Comparison of empirically derived ozone loss rates in the Arctic vortex, J. Geophys. Res., 107, D20, SOL 7-1–SOL 7-11, doi:10.1029/2001JD000482, 2002.

Hoffmann, L., Hoppe, C. M., ~~Mller~~Müller, R., Dutton, G. S., Gille, J. C., Griessbach, S., Jones, A., Meyer, C. I., Spang, R., Volk, C. M., and Walker, K. A.: Stratospheric lifetime ratio of CFC-11 and CFC-12 from satellite and model climatologies, Atmos. Chem. Phys., 14, 12479-12497, doi:10.5194/acp-14-12479-2014, 2014.

Lowe~~D.~~, D., and MacKenzie, A. R.: Polar stratospheric cloud microphysics and chemistry, J. Atmos. Sol.-Terr. Phys., 70, 13–40, 2008.

Rex, M., von der Gathen, P., Harris, N. R. P., ~~.~~Lucic, D., Knudsen, B. M., Braathen, G. O., Reid, S. J., De Backer, H., Claude, H., Fabian R., Fast, H., Gil, M., Kyrö, E., Mikkelsen, I. S., Rummukainen, M., Smit, H. G., Stähelin, J., Varotsos, C., and Zaitcev, I.: In situ measurements of stratospheric ozone depletion rates in the Arctic winter 1991/1992: A Lagrangian approach, J. Geophys. Res., 103, D5, 5843–5853, 1998.

Rienecker, M. M., Suarez, M. J., Todling, R., Bacmeister, J., Takacs, L., Liu, H.-C., Gu, W., Sienkiewicz, M., Koster, ~~5~~R. D., Gelaro, R., Stajner, I., and Nielsen, J. E.: The GEOS-5 data assimilation system – documentation of versions 5.0.1, 5.1.0, and 5.2.0, NASA Tech. Memo., TM-2008-104606, 27, 2008.