

Answers to B. M. Sinnhuber (Referee)

Thanks for your comments very useful for improving the paper.

General comments

*The title of the paper asks, if the ozone loss in 2010/2011 is related to climatic change. However, “climatic change” (or “climate change”) appears not to be well defined in this study. Not only greenhouse gases, but very critically also ozone, that is influenced by the amount of ozone depleting substances, have an impact on lower stratospheric temperatures, again influencing parameters like vortex strength. All of this takes place in the presence of large internal variability on inter-annual time scales. I suggest discussing these points in a bit more detail and more specifically in the introduction and for the conclusions of this paper.*

Title changed for climate. Some more details on possible climate influence added in the introduction and conclusion.

Introduction (p 313, l 11)

Although cold winters have been suggested to get colder during the last four decades (Rex et al., 2004) consistent with the global cooling of the middle and upper stratosphere predicted by chemistry-climate models due to the greenhouse gases increase (WMO, 2011), and a possible large depletion in case of unusually cold winter recognised for long (WMO, 2011), the eventuality of such extreme loss was unpredicted.

End of conclusion changed for

There is no sign of trend since 1994, neither in PSC volume, amplitude of denitrification in the early winter, renoxification of the vortex nor in total ozone depletion, but of high inter-annual variability. From the SAOZ observations there is no indication suggesting that the Arctic ozone loss has amplified or reduced since 1994. The only intriguing question is the reason for the unusual vortex strength in 2011 and if it could repeat in the future. This strength will highly depend on the further evolution of the winter Arctic stratospheric temperature. If warming in the future as predicted by most chemistry-climate models because of the increased wave activity compensating the cooling resulting from the increased levels of greenhouse gases (WMO 2011), the extreme 2011 ozone loss will remain a unique event and will not repeat. But the uncertainty on the further evolution of the temperature of the Arctic winter stratosphere does not allow predicting that it will not happen again in the future.

*2. The ozone loss diagnosed from the SAOZ total ozone observations in combination with the modelled “passive ozone” tracer has traditionally resulted in values considerably larger than in many other studies. Here you show, that roughly 50 DU of this loss are due to gas phase chemistry, and the remaining about 120 DU ozone loss agree well with many other studies. I suggest providing more details on this gas phase loss, in particular if possible on its dominant altitude region, its inter-annual variability and on possible differences between Arctic and Antarctic. This will greatly help to better understand differences between different methods to derive polar ozone loss.*

The contribution of gas-phase chemistry to O<sub>3</sub> loss in 2010-2011 has been quantified by Kuttippurath et al. (2012) with a version of the same Reprobus CTM integrated on isentropic levels. In their study, Kuttippurath et al. (2012) showed that the NO<sub>x</sub>-driven ozone loss cycles

were dominant in the stratosphere at 675K from mid-February 2011 onwards. Even at 475 K, the HO<sub>x</sub>-driven ozone loss cycles were the most efficient ones from early April 2011 onwards. Regarding inter-annual variability, these authors also showed that the net O<sub>3</sub> loss rate at 675 K was larger than in previous “cold” Arctic winters.

Altogether these effects explain the rapid increase in gas-phase loss that can be seen in the later part of the winter by the blue curve in Figure 2. Because most of the gas-phase loss occurs above 550 K, it is not surprising that our ~170 DU total O<sub>3</sub> loss diagnosed over the whole vertical column is larger than the 120 DU calculated by Sinnhuber et al. (2011) over the smaller 380-550 K interval. In fact, both studies are in good quantitative agreement if one considers that the ~50 DU difference is due to the high-altitude gas-phase loss occurring above 550 K.

The manuscript has been modified in paragraph 2.3 to give a better account of the points discussed above.

P 316, ligne 14

The calculated total ozone loss in the vortex at the end of the winter was  $38 \pm 5\%$  (approx. 170 DU), among which 12% (50 DU) would be due to gas-phase chemistry. Most of the ozone loss diagnosed by the gas-phase only simulation can be attributed to NO<sub>x</sub>-driven cycles that from mid-March 2011 are dominating the loss at upper levels, as shown at 675 K by Kuttippurath et al. (2012). This latter study also showed that the net O<sub>3</sub> loss at 675K was larger than in previous cold Arctic winters. Considering the fact that most of the ~50 DU gas-phase loss occurs above 550 K, the ~170 DU total loss over the whole vertical column diagnosed here is consistent with the 120 DU loss calculated by Sinnhuber et al. (2011) over the smaller 380-550 K interval.

Reference added

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

Specific comments

*p.312, l.21: PSC volume: more specifically, volume of air cold enough to allow formations of PSCs (or similar). Changed.*

*p.315, l.12: are “full chemistry” model calculations also available for comparison with the observations? They are available, but since a discussion of the model capacity to capture the loss is out of scope of the paper, our choice was not to show any full simulation of the ozone loss.*

*p.315, l.13: I suggest to formulate this more carefully, as I suspect you only speculate that these enhancements in winter 2010/2011 are from Western Europe.*

Not speculation. The Western and Central Europe origin of the pollution frequently observed over Sodanlya is explained in Pommereau and Goutail 1998.

*p.315, l.19: as noted above, I suggest putting more emphasis on the fact that comparison with “gas phase” run results in a 120 DU ozone loss.*

Done.

*p.315, l.22: what is inside the vortex in contrast to overpass of the vortex?*

Poor writing. Revised.

*p.316, l.14: as I understand, there are two factors: 1. differences in ozone loss between vortex edge and vortex centre, 2. sampling bias towards vortex edge in early winter.*

Changed for: During the second phase, when the sun reaches higher latitude SAOZ stations and the measurements are closer from the center of the vortex, the amount of data increases and the loss becomes more homogeneous

*p.316, l.15 was -> is*

Done

*p.316, l.17: can you give more details, what the dominant altitude region of the gas phase loss is?*

Gas phase dominant at high altitude above denitrified levels as shown by Kuttippurath et al. (2012). All paragraph re-written.

*p.316, l.23: or export of depleted air masses*

Added.

*p.317, l.8: Sinnhuber et al. (2011) also discussed denitrification from MIPAS/ENVISAT*

Added.

*p.317, l.25: potential volume (or similar)*

Changed

*p.317, l.28: why is this an indicator for the degree of chlorine activation? Ozone loss is related to sunlight, but chlorine activation is possible also in darkness!*

VPSC changed for proxy of activation and sunlit VPSC for proxy for CLO/chemistry

*p.318, l.1: PSCs could have formed*

Changed

*p.318, l.3 and l.7: again, chlorine activation does not require sunlight; the ozone loss cycles require sunlight*

Changed

*p.318, l.18: it is okay to use  $T_{NAT}$  as an empirical threshold temperature, but this does not provide sufficient evidence that actually solid NAT particles are doing the activation.*

*I suggest to formulate this more carefully or to discuss in more detail.*

Changed

*p.319, l.20: as noted above, I suggest discussing the gas phase loss in a bit more detail*

Changed for : caused by gas phase chemistry at high altitude of amplitude depending on the dynamics of each winter as explained by Kuttippurath et al. (2012).

*p.321, l.17: for curiosity: did you test, how well denoxification correlates with  $V_{PSC}$ ?*

Relative poor correlation. The denitrification is far larger in 2011 than on other years like 1996, 1997 of high on  $V_{PSC}$  or sunlit  $V_{PSC}$ .

*p.321, l.22: for 2011, about 50 DU loss are due to gas phase chemistry, presumably from higher altitudes. Did you test (a) if this value is similar, for different years and (b) if it is similar in Antarctica?*

Indication of inter-annual variability related to dynamics of each winter in the Arctic (Kuttippurath et al. (2012) but no information on Antarctic.

*p.323, l.5: the final sentence leaves me puzzled. What kind of information would you imagine, that would clearly suggest a reduction or amplification of future ozone loss?*

End of conclusion changed for some discussion of the impact of temperature predictions.

*p.328, Table 1: since when do these stations provide observations? All since 1994?*

Table revised.

Technical corrections

All taken.

*p.313, l.3: sonde and satellite observations*

*p.313, l.21: ozone loss*

*p.314, l.3: define SAOZ and NDACC when first used in Section 1*

*p.314, l.9: solar zenith angle*

*p.314, l.11: version V2 of what?*

*p.314, l.15: remove "also"*

*p.314, l.21: contradiction: line 18 says "without chemistry"*

*p.316, l.4: for inside and outside of the vortex, respectively*

*p.318, l.2: are of*

*p.321, l.1-4: split long sentence*

*p.321, l.20: move "with the passive ozone method" to the end of the sentence*