

Interactive comment on “Why unprecedented ozone loss in the Arctic in 2011? Is it related to climatic change?” by J.-P. Pommereau et al.

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Received and published: 3 March 2013

Arctic stratospheric ozone loss during winter 2010/2011 reached levels unprecedented in the Arctic, but comparable in magnitude to the loss occurring within the Antarctic ozone hole. Pommereau et al. use observations of total ozone and total column NO₂ from the network of ground-based SAOZ stations to investigate the ozone loss during winter 2010/2011 in comparison to previous Arctic and Antarctic winters. The long-term and bi-hemispheric context given in this paper as well as the discussion of differences in denoxification during this winter provides important new information. Pommereau et al. argue that from the long-term observations together with other diagnostics from meteorological reanalyses there is no indication for a long-term change suggesting that the unusually high ozone loss during winter 2010/2011 was related to climate change.

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The manuscript is generally well written and will make an important contribution to the discussion of Arctic ozone loss during winter 2010/2011 in particular and the relation between Arctic ozone loss and climate change in general. I recommend publication in ACP after consideration of the following comments.

General comments

1. 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.

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.

Specific comments

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

p.315, l.12: are “full chemistry” model calculations also available for comparison with the observations?

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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.

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.

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

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.

p.316, l.15 was -> is

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

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

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

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

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!

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

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

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.

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

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p.321, l.17: for curiosity: did you test, how well denoxification correlates with 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?

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?

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

Technical corrections

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

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 311, 2013.

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