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Interactive comment on "Record-breaking ozone loss in the Arctic winter 2010/2011: comparison with 1996/1997" *by* J. Kuttippurath et al.

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On page 6890, line 5-7, the authors point out the column ozone loss at the end of March in Balis et al. (2011) is half of that derived in other studies. We would like to point out that the column ozone loss mentioned in Balis et al., calculated with the Oslo CTM2, is a polar-cap average over the region north of 60N, and is not directly comparable to the

other references which are vortex averages. When the loss in CTM2 is averaged north of 70N equivalent latitude at 475K, we find a loss of about 95 DU by the end of March 2011. This is more comparable to the 112 DU calculated with Mimosa-Chem.

However, our estimation is not relying on the difference from a passive ozone tracer, but is calculated by comparing with an additional simulation where all chemistry is switched off north of 60N (gas phase and heterogeneous reactions on stratospheric aerosols and PSCs). The use of a passive tracer has limitations, because a passive tracer only represents advected ozone, and lacks production (mostly low latitudes) and loss terms (upper stratosphere), even in absence of PSC-related heterogeneous chemistry. Søvde et al. (2011) reported that a passive tracer should be used with care, and a passive tracer over 5 months will likely not be accurate for calculating ozone loss, especially after the vortex break-up.

It seems that production and loss rates are calculated in Mimosa-Chem, which could be used to calculate the total amount of ozone loss. Do you see any consistency with the ozone loss estimate based on a passive tracer?

References

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Interactive comment on Atmos. Chem. Phys. Discuss., 12, 6877, 2012.