

REPLIES TO THE COMMENTS OF Søvde et al.

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.

Thank you for this note. We have included this new analysis in the revised version. Please find the revised text in [Page 6, Paragraph 3, Lines 19–36](#)

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.

We calculate the ozone loss INSIDE the vortex. Therefore, inaccuracies in tracer values after the vortex break-up are not applicable in our calculations.

However, we admit that our tracer calculations have limitations and the ozone loss computations depend on the nature of tracer calculations (e.g. Singleton et al. (2005)). Thank you for reminding this point. We have revised our text in accordance with this point. Please find it in [Page 6, Paragraph 3, Lines 30–36](#)

There were some comparative studies in the past (e.g. [Harris et al. \(2002\)](#) present a detailed comparison of various ozone loss estimation for the Arctic winter 1999/2000, [Kuttippurath et al. \(2011\)](#) for the Arctic winter 2002/2003, and [Kuttippurath et al. \(2010\)](#) and [Feng et al. \(2011\)](#) for the Arctic winter 2004/2005) and they showed that, all methods yield very good agreement in deriving similar ozone loss values if they use/apply the same parameters in their calculations (such as the vortex edge criterion, data, and ozone loss computation method). The differences among the methods were about 5–10% and were well inside the error bars of the measurements used in those analyses. The studies also revealed that the passive method, that is used in this study, is a very good tool for estimating ozone loss inside the vortex. Therefore, this is a well-established and widely used method and was the reason to consider in this analysis.

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?

Our model simulations of ozone loss, and ozone loss and production rates are very consistent with those diagnosed by the passive method.

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