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Interactive Comment

Interactive comment on "Ozone loss and chlorine activation in the Arctic winters 1991–2003 derived with the TRAC method" *by* S. Tilmes et al.

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General Comments

This paper describes how ozone losses for the Arctic winters from 1991/92 to 2002/03 have been calculated using tracer and ozone data from HALOE. It is a thorough piece of work and it will be valuable to have a self-consistent set of ozone loss estimates produced by this approach. The discussion does seem dated in places and it could do with updating.

Specific comments It would be interesting to have a brief discussion of the interannual variability of the early winter reference functions shown in Figure 3, especially if the HF and CH4 could be de-trended. I think there is a general view / assumption that the November reference functions (vortex spin-up rather than the pre-ozone loss ones

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needed here) should be the same from year to year, but Figure 3 implies that there are actually significant interannual differences.

I do not recall that the ozone loss inside the 1991/92 vortex was homogenous and isolated (near the end of section 4.1, there is a paragraph starting ŚIn some yearsĚŠ). There was a major warming near the end of January which resulted in transport of air into the vortex reported by various authors (Pyle et al, 1994; Plumb et al.; Vaughan et al. 1994), and Lucic et al. (J. Atm Chem., 1999) stopped their analysis of ozone loss at that time. If the authors are to make that statement about that year, they need to justify it.

A couple of paragraphs later the authors imply that inhomogeneous temperature distributions must result in inhomogeneous ozone losses. This is not necessarily true as any inhomogeneity in the temperature is smoothed out in the resultant ozone loss because of the flow of air through the cold regions. 1991/92 is a good example of this.

I found section 5 hard to follow Ű perhaps its structure could be improved or made clearer. In the second paragraph (and some other places), I think the authors should give an upper limit on the ozone loss they think occurred rather than say it is zero. In the seventh paragraph, the rationale for differences with MLS is very vague. Why canŠt the authors integrate over the same range as MLS?

The least satisfactory part is the discussion in Section 6. There are some interesting results presented here but it is not really clear what the authors think they mean. Figure 13 shows a much looser relationship between APSC (Jan-Mar, 400-500K) and ozone loss (400-500K) than Rex et al (2004) find for ozone loss (14-24 km.) with VPSC (mid-December to end March for the range from 400-550K. This could be an important result and, if so, would justify the discussion of other important factors in section 6. First of all, however, the correct comparison should be with Rex et al (GRL, 2004) which is the published version of that study, rather than with the Rex et al. poster (2002) which was an early progress report of that study. Tilmes et al can of course choose to define quantities that they think are more appropriate, but they have (a) to analyse within estimated uncertainties whether the two approaches are giving significantly different answers as Fig 13 implies and (b) to know where any differences stem from. Neither of these points is addressed at present.

The discussion of early January losses in the Conclusions section (it should be retitled Discussion) also seems dated as there is no reference to the recent Rex et al GRL paper (2004) on the available measurements. To me the HALOE tracer technique seems to be the outlier as there is reasonable agreement between the other techniques.

Minor comments Given a similar approach has been used by others, I am not sure that it is worth giving this approach its own name (TRAC).

The start of the description of the 1995/96 winter in section 3.2 is a bit vague. ŞColdest recorded Ť needs to be made more precise in terms of the measure of coldness.

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