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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Vortex-averaged Arctic ozone depletion in the winter 2002/2003" *by* T. Christensen et al.

N. Harris (Referee)

Neil.Harris@ozone-sec.ch.cam.ac.uk

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This is a well written paper, presenting a sound analysis of ozone loss in the 2002/03 Arctic winter. I recommend that it is published. Chemical depletion rates between 350 to 525K are calculated from the ozone measurements made by the network of ozonesonde stations in the Arctic vortex. Trajectory calculations are used to take account of the vertical and horizontal transport of air within and into the vortex.

1. Uncertainties in transport terms (section 4) It is hard to assess the uncertainties associated with the transport terms, and I think a bit more discussion of the factors leading to uncertainties in the transport terms would be helpful.

Figure 2 does address the issue of uncertainty in the diabatic cooling rates. However the assumption in Figure 2 is that only diabatic descent influences the tracer isopleths,

which is inconsistent with the authors' recognition that horizontal motion also need to be taken into account. The vortex in 1999/2000 was significantly less disturbed than the one in 2002/03, so the assumption may be more valid in the former winter. At the moment the discussion on this point reads more like a justification of the calculations performed here rather than an attempt to give the reader a clear idea of the uncertainties involved. Incidentally it is not clear which model's results are shown in Figure 2 - this work's or SLIMCAT - or which vortex region(s).

In-mixing of lower mixing ratio air is (mathematically) equivalent to more descent and/or more chemical ozone loss, and picking these factors apart is the key to the approach here. At the moment, there is no discussion of the uncertainties associated with the trajectory calculations or the use of 10 day time steps. It would be helpful to show the size of the in-mixing term either in Table 2 or perhaps in Fig 5.

2. On p 6676, line 16-17, the author state "Small differences in the subsidence can give rise to large differences in ozone loss as the loss rate can vary significantly with height." I cannot find any justification for this statement in this paper (e.g. Tables 3 or 5), since any changes are no that large compared to the quoted uncertainties. The statement should either be removed or justified.

3. Some description of how the uncertainties in Tables 3 and 5 are calculated is needed.

4. Now that more papers on the 2002/03 winter are now on the ACPD website, the authors should add a paragraph discussing these results.

p. 6669, 14 formation rather than forming Table 2 It would be easier for the reader if the symbols for influx, and observed mixing ratio used in the text were repeated in this table as well as the legend.

Interactive comment on Atmos. Chem. Phys. Discuss., 4, 6667, 2004.