Atmospheric Chemistry and Physics Discussions

Interactive
Comment

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

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General comments

This is a well written paper presenting a topic of interest to the readership of ACP. The science is sound and the material is in general well presented. I suggest to publish a revised version of the paper that addresses some remaining concerns (see below).

My major concern regarding the paper is the treatment of the uncertainties. I suggest that all possible uncertainties (e.g., mixing in of mid-latitude air, errors in the decent calculations) should be considered and included in the reported error estimates. And such error estimates indeed should be reported, in the paper and in the abstract. Further, I suggest to be more explicit about the altitude range, over which the ozone loss

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is deduced.

Another problem is that there are by now other accessible estimates of ozone loss that should be compared with the values reported here as far as possible.

Comments in detail

Abstract Uncertainty estimates should be given together with the reported ozone losses. Further, the losses are actually calculated for a particular altitude (theta) range. This range should be given here.

line 17, p. 6669 variability of what?

line 19, p. 6669 There should be more citations here than just one.

line 4, p. 6670 probably 'large areas of possible PSC existence' (or similar) would be better here.

line 20, p. 6670 Why are the vortex average ozone loss estimates only compared to Match? There are ozone loss estimates available for this winter that are published in ACPD, also the study by Tilmes et al. (2003) reports ozone loss estimates for winter 2002-2003.

line 25, p. 6670 It would be helpful to discuss here briefly in how far this method of determining the vortex edge is different from the method introduced by Nash et al. (1996).

line 6, p. 6671 The discussion on the strength of the vortex should be extended to the theta levels in question here, that is up to 675 K.

line 4, p. 6672 Perhaps add a citation for the PV theta mapping? E.g., Lary et al. (1995) and references therein.

Section 4.1, p. 6672 The discussion here is in terms of vortex average descent rates.

What would be the impact of differential descent within the vortex (Ray et al., 2002) on the deduced ozone losses?

line 12, p. 6673 Quantify 'substantially'. Also, are the SLIMCAT descent rates those reported in Chipperfield (1999)? The reason for the different descent rates from SLIMCAT and Morcrette should be discussed.

line 20, p. 6673 tree → three

lines 9-21., p. 6673 The accuracy of the descent calculations is important for the technique employed here. I suggest to bring out more clearly how accurate the descent can be simulated, what the estimated error is, and how this error carries over to the ozone loss calculation.

Sec. 5/Table 4 I suggest that error estimates are given together with the values reported here. Further, the values should not be reported as column depletion, rather the vertical range 380-525 K should be reported . Finally, I am not sure if a table is needed to present the four numbers in Table 4.

line 2,3, p. 6676 This is not necessarily correct. Ideally, one would determine the vortex edge individually for each potential theta level in question. The fact that a scaling of PV removes most of the altitude dependence caused by the term $\partial\theta/\partial p$ in the definition of PV does not mean that the edge value for scaled PV is constant with altitude, indeed it depends on the specific scaling employed (see, e.g. the discussion in Müller and Günther, 2003, Fig. 2).

Table 5 Perhaps the vertical profiles of ozone loss reported here could also be shown as a Figure? And the comparison could be extended to other published estimates of ozone loss? Further, I would not recommend to list the error of the vortex average approach here as 1 DU.

line 15, p. 6676 How large is the difference in the deduced descent rates? How much effort would it be to do a test calculation of the vortex average approach with the descent rates employed by the Match study? In this way, the impact of the different

descent rates could be quantified.

line 7, p. 6677 Could a more quantitative statement than "not significantly" be made?

Figs. 1 and 4 Of course at 475 K the MPV as defined here is just the PV at 475 (i.e. the scaling factor is one).

Literature

- Lary, D. J., M. P. Chipperfield, J. A. Pyle, W. A. Norton, and L. P. Riishøjgaard, 1995: Three-dimensional tracer initialization and general diagnostics using equivalent PV latitude-potential-temperature coordinates. *Q. J. R. Meteorol. Soc.*, **121**, 187–210.

- R. Müller, and G. Günther 2003: A generalized form of Lait's modified potential vorticity, *J. Atmos. Sci.*, **60**, pp. 2229-2237.

- Nash, E. R., P. A. Newman, J. E. Rosenfield and M. R. Schoeberl, 1996: An objective determination of the polar vortex using Ertel's potential vorticity. *J. Geophys. Res.*, **101**, 9471–9478.

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