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Interactive comment on "Chemical ozone loss in the Arctic winter 2002/2003 determined with Match" *by* M. Streibel et al.

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General Comments

This is a very good paper. It describes in detail the results of an analysis of chemical ozone loss in the Arctic in winter 2002/2003 based on the Match technique. Match is an established technique for deducing chemical ozone loss in the polar vortex. An important strength of the paper is that it is not only applying the Match methodology, but that it is also extending the technique and is presenting evidence that the extensions lead to an improvement of Match, while establishing consistency with earlier Match analyses.

I feel the paper could be improved by discussing somewhat more extensively the impact of the findings for Arctic winter 2002/2003 on ongoing discussions on how the temperatures in the polar vortex are related to chemical ozone loss and on the question how well model simulations reproduce observed ozone loss rates in polar regions.

This is obviously a self-serving comment, but I believe that some discussion and comparison of chemical ozone loss deduced with the tracer-tracer correlation technique for Arctic winter 2002/2003 (Tilmes et al., GRL, 2003; Tilmes et al., ACP, 2004) to the results reported here would be helpful. Moreover, the model study of Grooß et al. (ACP, 2005) on the Arctic winter 2002/2003 might be of interest here.

In summary, this is an important paper that should be published in ACP after a revision taking into account the points made below.

Comments in Detail

Abstract: The abstract, as it stands is solely concerned with the Match analysis of Arctic winter 2002/2003. It would be of interest to include some information here on the impact of the ozone loss estimate for the Arctic winter 2002/2003 on more general issues, e.g. the question of the relation of chemical ozone loss in the Arctic to the area within the polar vortex below the PSC existence threshold.

Introduction: The mismatch between ozone loss rates as measured by Match and predicted by model simulations is an important motivation for the QUOBI project and thus for the present study. Therefore, I feel that more discussion of this point is warranted. For example, there are new developments since the Rex et al. (2003) study; recent papers by Salawitch et al. have suggested that ozone loss proceeds faster than assumed in the Rex et al. (2003) study.

Meteorology: There is some discussion on the meteorology of the 2002/2003 winter in Tilmes et al. (GRL, 2003) and a discussion of the meteorology of this winter in comparison to the Arctic winters since 1991 in Tilmes et al. (ACP, 2004) that might be

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helpful here.

Results: The vortex edge is defined by certain values of normalised PV. There should be some explanation on how these values were deduced, perhaps a discussion of how these values relate to alternative definitions of the vortex edge (such as the criterion of maximum wind speed or maximum gradient in PV), and some discussion of the sensitivity of the Match results in this winter to the particular choice made.

On top of P. 4318, error estimates for the deduced column ozone loss are reported. I assume that these estimates reflect purely the statistical error estimates and do not, for example, include the error due to the diabatic descent calculation. In any event, the basis of the reported error estimate should be clearly stated.

Uncertainty: Recently, Chipperfield et al. (GRL, 2005) have reported that a substantial change in simulated ozone columns is found in a model, when the diabatic heating calculation is updated. The question is if the 0.5/1.5 decrease/increase in heating rates is encompassing the change in heating rates introduced by the update of the long-wave radiation scheme in the Chipperfield et al. (GRL, 2005) model.

Summary: Definition of the vortex edge: It would be of interest to know, what the impact of different definitions of the vortex edge are. Further, I believe that some discussion of the studies on Arctic winter 2002/2003 by Tilmes et al. (GRL, 2003; ACP, 2004) and Grooß et al. (ACP, 2005) would be helpful here.

Figures: It is important to have the information from various sensitivity studies that the Match results are robust to details of the assumptions inherent in Match. However, in terms of presentation, I suggest that several of these sensitivity studies are better represented in difference (of ozone loss) plots rather than in plots showing several points lying (more or less) on top of each other.

MINOR POINTS

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- P. 4314, l. 7: why ‘approximately 30 stations’? The number of stations (34) is known.
- P. 4314, l. 9: Not everybody might know what a Match alert is.
- P. 4314, l. 19: add ‘previously’ after investigated.
- P. 4315, l. 2: define ‘Match radius’.
- P. 4315, l. 21: replace ‘they’ by ‘it’
- Caption of Fig. 2: cite Rex et al., 1999 for normalised PV. And perhaps add a few words in the text on the nature of this quantity.
- P. 4317, l. 16: Mention the details of the Match quality criteria earlier in the paper. Add a citation for a detailed discussion here.
- P. 4317, l. 26: This is the maximum of the observed acc. ozone loss – correct?
- P. 4318, l. 21: ‘cancelled the quality criteria’... unclear.
- P. 4319: ‘Uncertainty’ does not seem to be a good title of a section; Uncertainty of what?
- P. 4319, l. 8: replace ‘grid’ by ‘spatial’
- P. 4319, l. 10: ‘This files’ ?
- P. 4320, l. 17: Why 5 K resolution?
- P. 4322, l. : ‘Upper mentioned levels’ ? unclear
- Introduce a consistent capitalised spelling ‘Match’ throughout.

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 4311, 2005.