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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The impact of mixing across the polar vortex edge on Match ozone loss estimates" *by* J.-U. Grooßet al.

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This manuscript uses the CLAMS model to analyse the performance of the measurement-based Match technique in estimating ozone loss in the Arctic vortex during the 2002/03 winter. The first part of the paper is devoted to assessing the performance of CLAMS, with the authors concluding that ?The CLAMS simulation is able to reproduce the dynamics (mixing and advection) very well.? They then compare the ozone losses calculated with the model to those found by Match. They conclude that ?the effect of mixing across the polar vortex edge is important and should not be neglected in the ozone loss estimates? They go on to say that ? Although there was likely more mixing in Arctic winter 2002/2003 than in a typical Arctic winter, the problems with the Match analysis described here will in principle be present in all Arctic

winters.?

I have several problems with this manuscript and do not think it should be published in its present form. The main scientific issue is that the authors? faith in the CLAMS performance, while touching, is not fully justified in the material presented here or in the papers referred to (particularly Grooss et al., 2005 ? G2005). Further, little comparison/discussion is made with other model studies of that winter. My view is that the errors associated with the model calculated losses are on the low side and that the statements about the disagreement with Match are therefore too strong.

The presentation could be clearer ? it took me too long to really get to grips with their arguments, largely because of lack of clarity with quite a few superfluous points being made. This is not helped by the fact that it is not a new finding that Match is less good in regions of more disturbed flow (e.g. Kilbane-Dawe et al., JAC, 123, 2001 and I am sure others); rather the authors are trying to quantify this. This distinction tends to get lost. Finally there is a general sloppiness in the text of this manuscript indicating it has not been thoroughly proof-read by all the authors. Taking these points together, I think the real value in this work has been obscured by premature submission.

Major Points

1. How good is CLAMS at simulating ozone loss? The authors do show a number of comparisons with measurements. However I do not think that the uncertainties associated with CLAMS are properly discussed. The comparisons with measurements in this paper and the Groos et al (2005) paper are not as good as claimed and the importance of the differences should be quantified: e.g. the importance of getting the renitrification altitude wrong (Fig 4&5 in G2005); the offset at low N2O (Fig. 7 in G2005); and in Fig 1 in this paper the different slopes in the CFC-11/CH4 correlations and the wrong CH4 values outside the vortex. What are the chemical and dynamical implications of these?

2. How well do CLAMS and Match compare to other studies? There is still not enough

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discussion of other ozone loss estimates in this winter. There is now a somewhat preremptory paragraph in the Introduction and a table, but there is no reference to any of the other studies in the Conclusions. It is also hard to tell how CLAMS compares to other models studies

3. This manuscript should include a ?neutral? summary or discussion of the Streibel et al (2006) Match results for 2003/03.

4. What is the real point? It took too much effort on the part of this reader to understand properly the main arguments and to work out what the authors think is significant. Some statements are too sweeping. For example, the very final sentence of the Conclusions is ?It is also possible that a similar reported difference between CLAMS and Match for the winter 2004/05 (..) is due to very similar reasons.? I think that it is true (at least partly, but there is absolutely no evidence presented here to justify it ? not even a statement that the vortex below 450K or so in that winter was pretty disturbed.

5. What is ?mixing?? In terms of transport of air in and out of the vortex, the point is that Match measures ozone depletion along sets of trajectories which are sampled at both ends. Each Match thus contributes to an estimate of an ?instantaneous? loss rate. Assuming the Match selection criteria are working properly, the main uncertainties arise when averaging individual Matches over space and time. If the air being sampled is not homogeneous, this averaging is approximate at best. Various aspects of this have been examined in the past, including the uneven geographic distribution of the stations, different insolation (and hence ozone loss rates) at different latitudes. Influx of air into the vortex should not affect the Match estimate much if the Matches selected are all in the vortex. Flow out of the vortex causes problems because, in effect, ozone loss is exported, and what is important to remember is that the ozone loss in the actual air column in the vortex at the end of the winter is less than the integral of the loss that actually occurred within the vortex (by the amount exported). These aspects could be usefully be looked into and quantified here.

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Figure 8 is a good example of where the Match criteria did not work. However Figure 10 seems to show that the ?reduced? Match loss rates are in general larger lower down. In the text, the authors make a point about difference at the 500K level. It is not clear what they are doing here or how important examples such as shown in Figure 8 are. Are they only important for the two mentioned points at 500K? Are they counting such effects under the general term ?mixing??

Some of the points raised (e.g. the low sun angle effect) are actually pretty unimportant, so should they be included?

Minor Points The paper in its current form has too many typos and examples of unclear English. I am normally willing to point these out. However it is most definitely not the job of the reviewer to be a proof-reader ? it is the authors? ? and I am not doing so in this case.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 11725, 2007.