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Interactive comment on "Impact of mixing and chemical change on ozone-tracer relations in the polar vortex" *by* R. Müller et al.

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General This paper investigates the use of ozone-tracer relations to estimate polar ozone loss using a mixture of observations, model results and thought experiments. Some previous model results had indicated that ozone loss estimates deduced from ozone-tracer relation have significant uncertainties associated with atmospheric mixing and sampling. These studies are discussed and it is shown that while these are valid for the model conditions, they do not reflect real atmospheric conditions. Further the authors find that the ozone-tracer approach will work when (a) a good early vortex relation is obtained and (b) the vortex is well-defined in the sense that it is has an ozone tracer relation distinguishable from that at mid-latitudes.

The paper is the latest in a series of work on this subject by the authors, and I think it is fair to say that many of the basic ideas have been presented in one form or another elsewhere, so that the conclusions are to some extent unsurprising. However this paper presents them clearly in one place, it discusses the validity of the model papers of Plumb et al. (2000) and Sankey and Shepherd (2003) in a fair and insightful way, and gives a good 'feel' for how important the different mixing processes might be. The paper is well presented, well written and the arguments are strong. The subject matter is well within the remit of ACP. I recommend it for publication with relatively few changes listed below.

Specific comments 1. Ozone-tracer relations have been studied using Eulerian chemical transport models driven by meteorological analyses. I would expect these to show different relations to GCMs such as CMAM and they should be referred to in 2.2.1. For example the ozone loss deduced chemically in SLIMCAT is compared to the ozone loss deduced from changing tracer relations in SLIMCAT in Robinson et al. (2005), the point here being that realistic compact relations can develop in Eulerian CTMs such as SLIMCAT. At the moment the discussion is framed too strongly as Eulerian vs. Lagrangian.

2. The authors have thought so carefully about the use of ozone-tracer relations to deduce ozone loss, that it would be interesting to know what lessons can be applied to other areas of atmospheric research where changes in tracer-tracer relations are used. I realise that this is a rather general request, but if it is possible it would greatly increase the general relevance of the paper.

3. Are there circumstances (e.g. during major warmings) when the effective diffusivities are higher than those found in Waugh et al. (1997) or used in CLAMS so that the assumption of Plumb et al. (2000) is more valid?

Minor

5843, 10: 'Ĕused for more than a decade to quantify..' 5846, 17-22: I am not sure why

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this is being pointed out. 5848, 1-8: this should be rewritten - it is not clear. 5849, 15-16: also unclear: presumably the CMAM and observed relations at high altitude are in better agreement as they are representative of outside vortex air. 5850, 1-2: ‘Ěconclusion that the apparently compact correlations in the vortex may result from insufficient samplingĚ..’ 5857, 27: ‘Ěcompact for a chemical reasonĚ’ 5859, 2: ‘result’ rather than ‘remainder’

1. The words ‘very’ and ‘moreover’ are used a lot which I personally find unnecessary. 2. I still find the nickname TRAC annoying rather than helpful. 3. There are a few ‘to be submitted’ references which have to be sorted out.

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 5841, 2005.