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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Ozone loss and chlorine activation in the Arctic winters 1991–2003 derived with the TRAC method" *by* S. Tilmes et al.

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General comments

This paper is a very comprehensive overview of ozone losses derived with the tracer relation approach for the past 12 Arctic winters. It discusses in full detail the approach for each individual winter, the meteorology of each winter and the results that also include information about the degree of chlorine activation. Since the way how the early winter reference relation is defined varies widely from year to year (ranging from using a climatology over early winter HALOE measurements to mid-winter in-situ measurements) the data set is not really fully homogeneous and this should be pointed out in the abstract. But given this caveat this comprehensive overview is very valuable for the community and should eventually be published.

However, I have significant problems with section 6 of the paper. These concerns are described in detail in my comments below. I feel these concerns need to be addressed before the paper can be published. Since the rest of the paper stands on its own and deserves publication even if some conclusions of section 6 should not survive the revisions, I am optimistic that a revised version can be recommended for publication in ACP.

Specific comments

p2168 l 5: I am afraid that introducing the term TRAC for the tracer relation approach will lead to considerable confusion in the future. The term "tracer relation approach" is well established and many people have used this approach and will continue to use it. I doubt that they will adopt a new name for it. I would recommend to stick to the established term.

p 2168, l 14: The general term "are in agreement" is a bit problematic here. Some results agree well with previous analyses, others do not agree that well. For example the study reports larger ozone loss in winter 1991/1992 compared to the loss in winter 1999/2000, which is not in agreement with the general impression one gets from the literature published so far. Also, for individual years the differences between results of this study and previously published results are sometimes quite large, e.g. for 1997 they exceed a factor of two (this study compared to SAOZ/REPROBUS, table 7). These differences are not only due to strong January losses in the SAOZ/REPROBUS data.

p 2168, l 23-28: This conclusion is not consistent with previously published results (Rex et al., 2004) and is not supported by CTM calculations that reflect our current understanding of the ozone loss mechanism: the updated version of SLIMCAT (Chipperfield, presentations at the Quadrennial Ozone Symposium, 2004) reproduces the compact relation between ozone loss and Vpsc as published in Rex et al. very well and the compactness of the SLIMCAT relation between ozone loss and Vpsc also agrees well with those observations. I am not convinced that the deviations from the compact dis-

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tribution in this study (claimed to be about 40 DU, but I think it is closer to 20 DU, c.f. my comments about section 6) exceed the uncertainty that is connected with the approach. One has to keep in mind that the discrepancies between the results from this study and previously published results regularly exceed 30 DU. Interestingly the deviations from the compact distribution are largest for the years when the discrepancies between the results from this study and previously published results are largest.

section 2.2: I am missing a discussion of the possible quantitative effect of mixing during early winter. It has been shown in many studies that between about January to March mixing ratios of tracers inside the vortex are not much impacted by mixing accross the vortex edge. But there are indications (e.g. Ray et al.) that mixing accross the vortex edge and intra-vortex mixing following differential subsidence in the vortex do change tracer relationships inside the vortex during November/December. My feeling is that these dynamical processes contribute substantially to the uncertainty of the approach.

section 3.1, first paragraph: Since mixing during the early vortex period is a potential issue, the dates when the early winter reference profiles have been measured has to be given for each year. These dates probably vary significantly, depending on the UARS orbit and the vortex size and location.

p 2178, l 18-23: That nicely illustrates the problem: between November and January ozone mixing ratios at a given CH_4 level increase substantially due to dynamical effects (not only mixing accross the vortex edge but also due to differential subsidence followed by intra-vortex mixing). Therefore the reliability of the approach depends crucially on the availabilty of a late December or early January reference relation. I think the dates of the reference relations are important and need to be given (see my previous comment). I suspect that for most years for which the reference is based on HALOE measurements the reference was measured relatively early during the winter which will lead to larger uncertainties of the results for these years (risk of underestimating the loss).

p 2178, l 24-25: The black crosses are only about 0.4-0.5 ppmv below the reference relation (not 1.2 ppmv) ?! Perhaps I am missing something here ?

section 3.1, last paragraph: Given the large variability of the early winter reference relation as seen in Fig 3, it seems to be very adventurous to use a climatological reference relation for 1997-1998 and 2000-2001. How large (in DU) ist the variation of the loss estimates for these two years when the calculation is done with any of the individual reference relations ? That should be a fair estimate of the uncertainty of these results.

p 2197 l 9/10 and 15/16: The reference Rex et al. 2002 (a poster) should be replaced by Rex et al. 2004 (the corresponding updated paper in GRL).

Section 6: I have the following substantial problems with this section:

1. As outlined above, I think the uncertainties of the approach are probably quite a bit larger than reflected in the error bars (its my understnding that these do not include the potential effect of mixing in early winter). Even with the error bars currently given in the plot, a linear fit through the data would go through all error bars. Hence there is no significant deviation of individual points from the linear relation and I can't see the basis for the discussion in this section. The impression that individual points deviate from the linear relation only comes from the black line currently shown in Figure 13. What is that black line ? The text says "possible linear relationship". But it is definetly not a fit through the data, because basically all data points fall above the line. Was that line just drawn by hand ? This is quite worrying, because the whole section is based on the deviation of individual data points from that line, which seems to be arbitrarily chosen and the deviations from a real fit are much smaller (and not significant).

2. It is hard to see any correlation between "Sun hours per day at Apsc" and "ozone loss anomaly" in Figure 14. What is the correlation coefficient (I am sure it is quite small and not significant), what is the slope of a linear fit through the data (it is probably nearly flat, perhaps even slightly negative). For me it is hard to comprehend the statement in

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the text "A linear relation between the ozone loss anomaly and sun hours per day at Apsc is obvious". Again the reader is easily misled by the black line in Figure 14. What is the basis for this line, where does it come from ? I guess this is an attempt to already consider the Pinatubo effect that is discussed at the end of section 6. If the years 1992, 1993, 1994, and 1995 are disregarded, one could indeed see a slight correlation in Figure 14. But, first, this comes only from 1998 and 2003 which show slightly larger "ozone loss anomalies" than the other years. But these deviations are within the error bars. For 1998 the early winter reference was constructed from a climatology - an approach that will lead to large uncertainties if it is a valid approach at all. Second, the discussion about the Pinatubo impact on the "ozone loss anomaly" is not consistent. The "ozone loss anomaly" in 1995 is as large as in 1993, but it is very small in 1994. Hence, the Pinatubo effect does not seem to be a consistent explanation for the large positive anomaly in 1995. If 1995 is not disregarded, the slight correlation between "Sun hours per day at Apsc" and "ozone loss anomaly" largely disappears.

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