

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

S. Tilmes et al.

Received and published: 22 July 2004

General comments We thank the referee 1 (M. Rex) for critical but very helpful comments mainly on chapter 6 that helped to improve the paper. A more precise analysis is now performed in this chapter to illuminate the reason for agreements and disagreements between results of the vortex average approach and TRAC (see new Section 6). Owing to this analysis and re-discussion of the results the problems the referee has raised will be addressed in the revised manuscript and are additionally discussed in this comment. Further, a new figure is added in the revised version: Figure 1. This figure shows the “daily sun hours at possible PSC areas over the entire polar vortex, as a function of altitude, for the time period from November to April for the twelve winters between 1991–92 and 2002–03. This figure indicates the regions, where the area of possible PSC existence is zero and further, the time (interval) for which the early winter reference function was derived.”

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This Figure helps to understand many aspects that were not expressed clearly enough in the originally submitted manuscript.

Specific comments

Referee 1: *...the data set is not really fully homogeneous and this should be pointed out in the abstract*

In the abstract, a new sentence is added:

“HALOE measurements do not cover the polar region homogeneously over the course of the winter. Thus, to derive an early winter reference function for each of the twelve years, additionally, all available measurements were used; for two winters climatological considerations.”

p2168 line 5:

The name “tracer relation approach” is also not the only name of this method. In the literature names like: “tracer-tracer correlations” Tilmes et al. (2003), “correlations between CH₄ and Ozone” Müller et al. (1997), “HALOE ozone tracer relations” Harris et al. (2002) were used.

To prevent confusion in the future the name TRAC method will be changed to ‘Tracer-tracer Correlations’ in the title and the shortening will be only used as an abbreviation in the text.

p 2168, line 14:

Results of this study are generally in agreement with previous studies. We agree, that there are some comparisons with other methods that are not in agreement. This will be specified in more detail in the abstract:

“Results derived here are in general agreement with the results obtained from other methods to deduce polar ozone loss. Differences occur mainly owing to different time periods considered in deriving accumulated ozone loss. However, very strong ozone

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losses as deduced from SAOZ for January in winters 1993–94 and 1995–96 can not be identified using available HALOE observations in the early winter.”

We don't want to go into more detail of differences between different methods, because this is the abstract of the study. These large January losses as deduced from SAOZ are mentioned, because this is the most significant difference compared to the HALOE results. These differences cannot be explained in this study (specially for the winters 1993–94 and 1995–96) due to different time intervals used to calculate ozone loss, but only due to the fact that only HALOE profiles inside the vortex boundary region are available.

Further differences are already explained in the specific sections (or in the new Section 6), for example for winter 1991–92: New Section 6:

“One reason for the discrepancies between the two approaches may be that Rex et al. (2004) started their ozone loss calculation at day 15 (25) of the year in question. This is about one month later than the start of ozone loss calculation using the TRAC method (Figure 1). Especially in 1991–92 a large PSC area and significant solar insolation already exists before day 15 of the year 1992.”

Difference in 1991–92 between the TRAC method and MLS also occur owing to a later start of ozone loss calculation derived using MLS observations.

p 2168 ine 23-28:

Referee 1: *The 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...*

In view of the referees comments and owing to further analysis of the ozone loss and the meteorological conditions during the Arctic winters in question, our conclusions from Section 6 have changed somewhat. Most importantly, our findings are *not* in disagreement with the study by Rex et al. (2004). Indeed, in the new Section 6 it is

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stated now very clearly that our conclusions are in agreement with previously published results (Rex et al., 2004), if considering the same averages of the potential volume of PSCs. Thus also the CTM results are in accordance with the results presented here. The slope of SLIMCAT results (Chipperfield, presentation at the Quadrennial Ozone Symposium, Kos 2004) even seem to fit somewhat better to HALOE results. However, this issue is not discussed in the paper.

If the same time intervals are chosen to average the possible volume of PSC existence as the one for which the accumulated ozone loss was deduced, also another factor important to ozone loss processes is becoming noticeable, the influence of solar insolation. Indeed, this reflects our current understanding of the ozone loss mechanism, because solar radiation is involved in ozone destroying cycles. These results will be put forward very, clearly in the revised version of the abstract.

The discussion about uncertainties of ozone loss estimates derived using the TRAC method will be discussed below. However, results in the new Section 6 show that the impact of the uncertainties of the TRAC method on the conclusions is much less dramatic than described by the referee. In view of the comments, however, we have reduced the weight given to the more uncertain ozone loss estimates (in winters 1997–98 and 2000–01) in the discussion.

Section 2.2: The uncertainty of the early winter reference functions:

To determine the early winter reference function, only these profiles were used that were located poleward of the edge of the polar vortex using the Nash algorithm, as described in the submitted manuscript (Page 2172, line 20). Therefore, an isolated vortex (with a defined vortex edge) was already established. Mixing is possible, if the vortex edge is getting very weak. This is discussed in Section 2.1:

“Horizontal mixing across the vortex edge may change the tracer-tracer relation without chemical change. A case in point is the winter 1996–97, where the ozone-tracer relation changed until the beginning of January 1997, due to horizontal mixing pro-

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cesses (Tilmes et al., 2003). Further, an analysis of HALOE and balloon observations show that in winter 1991–92 the ozone-tracer relation has changed from November to December 1991 due to mixing.

In summary, the early winter reference function has to be determined at a time when the vortex has already formed and, additionally, is sufficiently isolated from mid-latitude air, but at the same time early enough so that no ozone loss has already taken place. Therefore, if the vortex is isolated, the reference function has to be derived as early as possible, if observations are available, to ensure that no ozone loss has already occurred.”

We will emphasize more clearly in the relevant paragraphs that the vortex was isolated at the time when the reference function was derived. At this point we will add the following sentence: “These conditions are generally fulfilled for each of the derived reference functions and some exceptions will be discussed in detail below.”

Further below in the revised manuscript we will discuss the isolation of the early winter: “Mixing processes may change the early winter reference function without chemical change if the vortex is not isolated. However, a significant increase in the uncertainty range due to mixing processes in the early vortex is not expected, because each profile used to derive an early winter reference function was located poleward the vortex edge (using the Nash criterion). The vortex was isolated for most years considered at the time when the reference function was derived. This can be assumed regarding the evolution of calculated PV values at the vortex edge using the Nash criterion. At the time when the reference function was derived, PV values at the vortex edge are 30–35 PV-units ($1\text{PVU} = 10^{-6} \text{ K m}^2/(\text{kg s})$) at the 475 K level for all year. In the following two weeks, PV values are increasing in the most of the years at the 475 K level (except for the winter 1998–99). Therefore, the uncertainty due to dynamics on the reference function should be small in all the years considered. In 1998–99 the vortex was less strong although still isolated; in this winter, a stronger influence of mixing on the early vortex reference cannot be excluded.”

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Further a measure of strength of the vortex can be derived, summarising these days of each year over the entire winter, when the poleward boundary of the vortex (as defined by the Nash et al. (1996) criterion) exceeds a certain threshold value of PV. (See reply to the comments of referee 2.)

Section 3.1, first paragraph:

The new Figure 1 will be added to the revised manuscript as it will help to clarify the discussion about the early winter reference function and further, the results in the presented Section 6.

The time when the early winter reference functions were derived, differs between the middle of November until towards the end of January. Nevertheless, as described above, the vortex was always well isolated at the time of the year in question.

p2178, line 18–23: Referee 1: *I suspect that for most years for which the reference based on HALOE measurements the reference was measured relatively early during the winter which will lead to larger uncertainties of the results...*

As explained above this is not the case. Further, Table 7 of the new and submitted manuscript already indicates the time when the reference function was derived.

p 2178, lin2 24-25:

In response, the sentence is changed to:

“The HALOE profiles in January 1992 scatters below the derived reference relation, most significantly at lower altitudes around the 1.2 ppmv CH₄ level (see Fig.).

Section 3.1, last paragraph:

We agree, that this calculation, based on climatology is of course just a rough estimate. Of course, results would change enormously, using for example the 1991–92 reference function. Nevertheless, this reference function is the average of all the de-trended early winter reference functions and therefore quite reliable within the reported range

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of uncertainty.

In the revised manuscript, we will add the information of a large uncertainty of ozone loss in these two years in Section 2.2: Error analysis.

p 2197, line 9/10 and 15/16

citation will be changed to Rex et al. GRL, 2004.

Section 6: To address this problem, we considered a different time interval to derive the volume of possible PSC existence, from the time when the reference function was derived to the end of March (February if no March ozone loss could be calculated).

These changes result in a much clearer dependency between ozone loss anomaly and daily sun hours. This conclusion is not based on rather uncertain results (winter 1997–98) and is not a result due to uncertainties in ozone loss estimates. Further, the dependency between ozone loss anomaly and daily sun hours was already found considering the early ozone losses: 1991–92 compared to 1995–96. These two years differ significantly in the amount of illumination in the early vortex and different amounts of ozone loss were found (page 2178, line 18-23 of the submitted manuscript). The linear relationship between V_{PSC} and ozone loss was fitted through all values, now. Further the linear relationship between ozone loss anomalies and daily sun hours is fitted for all years except for 1991–92 (Pinatubo effect) and 2000–01 and 2001–02 (uncertain results). Neglecting of the winter 1998–99 (rather large uncertainty) would not alter the deduced linear relation.

In summary, the conclusions of the analysis of the relation between the meteorological conditions and ozone loss have changed somewhat; most notably the discrepancy between our conclusion and those of the study by Rex et al. (2004) has been largely resolved. The entire text of the paper will be changed to reflect this change.

Interactive comment on Atmos. Chem. Phys. Discuss., 4, 2167, 2004.

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