Second Review of "Stratospheric ozone loss in the Arctic ..."

BY GRIFFIN ET AL.

General remarks

The authors have done extensive revisions of the paper in response to the comments received from all three reviewers. I think that the paper has been improved very much. I think, however, that some of my original concerns have not been fully addressed in the revision (see below). I have also some further comments when reading the new version of the paper again. I apologise that not all of these points were clear to me when writing the first review.

I recommend a further revision of the paper and the improvement of the discussion, as outlined below.

Discussion

It is stated in the abstract (and the conclusions) that the average vortex profile descent technique leads to smaller maximum losses. It would be good to quantify this statement somewhat. For example saying something like "by about 15-20 DU"

It is also stated in the abstract that the passive subtraction method using CTMs "results in smaller uncertainties...". However, the reported error bars clearly only cover some of the processes contributing to uncertainties. The CTMs are driven by ERA-I, using a different reanalysis would certainly result in a different vertical transport and thus in different passive ozone profiles. This issue should be discussed in the section on the method (3.4). Further, this method should also be influenced by the 2-5% difference between MLS and ACE-FTS ozone (p. 11). It would also be helpful to estimate the magnitude of this source of uncertainty and report this in section 3.4.

The paper correctly points out that mixing across the vortex edge is an issue for the tracer-tracer method. However, this issue is also implicitly present in the other methods. For example it is not clear how well CTMs represent mixing across the

vortex edge compared to reality. They are very likely not prefect in this respect and therefore will also likely misrepresent to some extent the impact of mixing on passive ozone. I suggest that the impact of mixing across the vortex edge is discussed for all methods used here (section 3).

A focus of the present paper is the chemical Arctic ozone loss in winter 2010/2012. The authors have provided a good coverage of the literature on this case. For this winter, Isaksen, et al. (GRL, 2012) report that "weakened transport of ozone from middle latitudes, concurrent with an anomalously strong polar vortex, was the primary cause of the low ozone. When the zonal winds relaxed in mid-March 2011, Arctic column ozone quickly recovered". The authors might consider to also discuss this facet of polar ozone in winter 2010/2011 in their paper.

In the summary and conclusions it is pointed out that CFC-11 has limited coverage compared to other species and therefore should not be used to estimate ozone depletion. It is not clear however, why this problem does not carry over to the artificial tracer method (discussed just below in the summary). I suggest more discussion of this potential problem.

At the end of the summary and conclusions (p, 20, l. 18) it is stated that the recommendation is to use the passive subtraction technique for years with an unstable polar vortex. This statement (together with table 1) leaves me with the problem that the two CTMs lead to different results for a more stable vortex (e.g. 2005); which model should I believe for 2005? The reason why the passive subtraction technique is recommended (p. 20, l. 18) is that the method took into account mixing across the vortex edge, this should also be okay for 2005, when this mixing was likely less strong.

Finally, I respect that the authors make a different assessment of some of the literature than the reviewer. However, Michelsen et al., (1998b) use an outside vortex reference to discuss the results of the tracer-tracer method; this is clearly not appropriate (and is not done in this paper, see section 3.1). Plumb et al., (2000), in their (very diffusive) conceptual model ignore the fact that there are two very different ozone tracer correlation inside and outside of the vortex edge. These two studies (and the corresponding discussions in review papers) are therefore of limited help when addressing the impact of mixing across the vortex edge on tracer-tracer relations.

Points remaining from the first review

I stated in my first review that I suggest improving the discussion of the issue that the tracer-tracer and the artificial tracer methods neglect descent from high altitudes; in particularly from the upper stratosphere and mesosphere. In response, the authors have changed the discussion and pointed out that this effect is not relevant for the height range of interest in the paper (see also their response to my previous comment on p. 10, l. 9).

I agree, but I still think it is necessary to clearly state in the paper that none of the methods discussed here does account for intrusions of mesospheric air. Perhaps I am wrong, but then the mechanism of how descent from high altitudes is included should be given in the description of the methods. Indeed I could only find one paper (Muller et al., JGR, 2007) where the issue of intrusions of mesospheric air into the polar vortex has been discussed in the connection with polar ozone loss estimates

I pointed out in my first review that for the winter 2004/2005, the comparison between the SLIMCAT simulations (SLIMCAT only and SLIMCAT) and the estimated ozone loss from the descent method is and 67 ± 3 DU 47 ± 4 DU. (Are the authors sure that exactly the same value is correct here for SLIMCAT only and SLIMCAT?) Thus these two methods do not agree for 2005 and 2005 was not a particularly disturbed vortex. However, the two tracer techniques, for 2005 (Table 1) agree within error bars with all methods, both for mean and max (with the exception of Decent, max). Therefore I am confused about the statement in the abstract that "the tracer-tracer correlation method does not agree with other estimation methods in March 2005". This statement seems to be in contradiction to the information given in Table 1.

Details

- p. 2., l. 5: not only "include"; these are all the methods. The same is true for the conclusions.
- p. 2., l. 8: "From the seven tracers measured by ACE-FTS...' Perhaps even list the seven tracers.
- p. 2., l. 16: add 'using the different methods" after "loss"

- p. 2., l. 33: change to “primarily ice particles”
- p. 3., l. 1: Here you need to drop “ice” (which does not form at 195-197 K)
- p. 7., l. 21/22: drop “and stable”
- References: Coy et al: title should not be in capital letters
- References: Michelsen et al., 1998 should have a,b in the reference list
- References: Sheese et al., 2016 is listed as “in press”