Atmos. Chem. Phys. Discuss., 2, S1093–S1097, 2002
www.atmos-chem-phys.org/acpd/2/S1093/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "The impact of mid-latitude intrusions into the polar vortex on ozone loss estimates" *by* J.-U. Grooß and R. Müller

J.-U. Grooß and R. Müller

Received and published: 27 March 2003

We thank both referees for their comments and suggestions. Especially the comments of the reviewer Markus Rex led to additions and improvements of the simulations beyond his request. He pointed out the importance of the trajectory selection criteria used in the Match analysis and suggested to view these simulations without these criteria as an upper limit for the Match error. Instead, we incorporated those criteria to the analysis presented here. The conclusions from that addition have changed: Indeed the Match trajectories "influenced by mixing" were sorted out by using these criteria and no significant bias on the ozone loss rates was simulated.

In addition we fixed a small mistake in the "virtual Match" trajectory calculation. Here, accidently the wind data were used every 24 hours in the submitted manuscript instead of the intended 6 hours. This was corrected in the revised manuscript.

1. Specific answers to the comments of referee # 1 (M.Rex)

On General Comments:

1. We did re-organize the revised manuscript such that the simulation for the both methods are now given a subsection each. This should clarify now the "mixing between parts".

2. We have added a figure that shows the vortex average ozone mixing ratios for the time dependent definition of the vortex edge after Nash et al. (figure 6). It shows that for this definition of the vortex edge the obtained results are very similar.

3. We have also (beyond the request of the reviewer) added an investigation on the effects of the Match trajectory selection criteria on the Match results. Indeed, the simulation shows, that these criteria are suited for sorting out the trajectories "influenced by mixing".

4. We focus especially on this period for two reasons. Firstly it is the period where up until now the largest discrepancy between Match and box model simulations was found. Secondly, it is a highly disturbed case with large-scale intrusions into the vortex, where one would rather expect difficulties in diagnosing ozone loss with the two methods. Of course it may be interesting to repeat this study for other winters with an isolated and homogeneous vortex. But we think this is beyond the scope of this paper. Given the results shown in the revised manuscript, we would expect rather no disagreements for these more regular winters.

On Specific Comments:

On comment 2490, 10-14 The abstract has been rewritten in the requested sense. It now covers also the additions to the paper mentioned above.

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On comment 2492, 15-24 We admit that the discussion about figure 3 may be misleading. The figure was meant to illustrate the size of simulated filaments and the Match radius. It does not contain significant information that was not in figure 2. Therefore we did remove the old figure 3 and the corresponding discussion from the revised manuscript.

On comment 2493, 25- 2494,20 This hole paragraph has been rewritten such that the two different investigated approaches are now discussed in different sections. As indicated above, the basic features of Match which are also the selection criteria have been incorporated into the analysis. With the re-organization of the paper and the additions we have accounted for this valuable criticism.

On comment 2495, 21-23 We agree and have left out the misleading comment in the brackets.

On comment 2496, 15 We agree and have rewritten and rearranged the text as suggested.

On comment 2496,13 - 2497,5 As mentioned by the referee, we now include two of these 3 selection criteria.

On comment 2497, 6-18 We do not fully agree with the critics to our method, especially not with the comments on the mixing parameterization. The mixing parameterization of CLaMS was not tested against the physical mixing processes, because this would be an impossible task. However, it was tested in an integral way for air masses that have experiences both high and low wind shear over a period of weeks [McKenna et al., 2002, Konopka et al., 2002]. Filaments that are produced in a high wind shear region, can be transported in a quasi laminar flow without mixing for a long time. The mixing parameterization was tested in that way against satellite observations (CRISTA) and aircraft in-situ observation (ER-2 during SOLVE).

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We think the main point why the variability of ozone within the vortex may be not well represented is the uncertainty in the initial ozone field that was derived from MLS data. Also, the mentioned ER-2 observations of vortex air during comparable period in January 2000 do indeed show variability of the order of 10%. If anything than we expect the variability to be somewhat too low. This can be seen by the additional figure 4 that shows the simulated and observed ozone mixing ratio by 123 ozone sondes.

Further, we do believe that differential subsidence should not have a large impact given the short simulation period. Some aspects of this criticism have been clarified by the re-arrangement of the manuscript.

Therefore we do believe indeed that with this study we make a reliable estimate of the statistical error that occurs due to the small number of matches.

On comment 2497, 20-27 The conclusions that have changed due to the addition of the trajectory selection criteria have been rewritten.

2. Specific answers to the comments of referee # 2

On comment 1 Some of the studies the referee asked for were already mentioned, but not clearly in the context of the model validation. We have now added a sentence that lists the most relevant publications with respect to model validation.

On comment 2 MLS data are already used for initialization and not much other data are available during the simulation period. We do show now an additional intercomparison with ozone sonde data between January 17 and 31 (figure 4). We think that this gives a good impression of the model performance during the simulation period.

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On comment 3 We did add some background about the January ozone loss discrepancy. There is much discussion on this subject. However, most searches for a solution of this discrepancy (including the efforts of the authors) were not successful. Thus, to our knowledge, very little is published on that subject.

On comment 4 We did mention the speculations about an unknown process at high solar zenith angles in the revised manuscript.

On comment 5 We agree. This statement was meant for the considered period only and should not be understood as a general valid statement. We changed the wording to avoid this mis-understanding

On comment 6 The intrusions in the considered time period are very un-typical for this time of the year. We think that it is critical to investigate both methods for this highly disturbed cases, because difficulties may arise from these intrusions. With the changes in the revised manuscript we show that these large-scale intrusions especially have no significant impact on the Match analysis. See also point 4 on the reply to referee 1.

On specific comment 2490, 6 This study focuses on the winter 1991/92, because during this winter the discrepancy between simulated and experimentally derived ozone loss rates is reported to be the largest. This was added to the abstract.

On specific comment 2491, 24 We see these intrusions in the CLaMS simulation, but it was first shown by Plumb et al. (1994)

The other specific comments have been changed as suggested by the reviewer.

Interactive comment on Atmos. Chem. Phys. Discuss., 2, 2489, 2002.