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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Investigation of Arctic ozone depletion sampled over midlatitudes during the Egrett Campaign of spring/summer 2000" *by* D. E. M. Ross et al.

D. E. M. Ross et al.

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We thank the three reviewers – Anonymous reviewer 1 (AR1), Rolf Müller (RM) and Jens-Uwe Grooss (JUG) – for their positive recommendations and constructive comments.

General comments

All three reviewers state that the results are not presented as clearly as they would like, and we have revised the manuscript to improve this. They have also given many useful references most of which have been added to the paper. Their general comments have all been taken into account or clarified.

The general comment (by AR1 and RM) concerns the role of mixing and our discus-

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sion of it. The points made by RM are now mentioned in the paper and significantly strengthen that discussion. We now make it much clearer that the role of mixing is taken into account as AR1's statement that we do not is based on a misreading.

Anonymous referee 1

The referee makes three general points.

(i) The influence of mixing We think this results to a large degree from a misunderstanding as we think that the referee is suggesting what we have done. In the paper we calculate ozone loss based on the observed ozone-tracer correlation and then use the model to investigate the validity of this approach, to diagnose the chemical ozone loss cycles and to look at the hypothetical April flight. In the light of the misunderstanding, we have clarified the text also taking into account the comments of Rolf Müller.

(ii) Model/measurement agreement The model does not capture O_3 mixing ratios in the mid-/low latitude air around the filament. The measurements are made near to the lowest level of the SLIMCAT model where the ozone mixing ratios are effectively prescribed. The good agreement between model and measurement in high latitude air results from the relatively strong influence of higher altitude air as a result of descent through the polar winter. The poorer agreement in mid and low latitude air results from a stronger influence of the bottom boundary. The focus of this study is the diagnosis of the ozone loss occurring in the high latitude air using measured ozone-tracer correlations and so this should not affect our results.

We think that the relative constancy of the difference between passive ozone and observation in Fig. 3c results from a coincidental offset between greater ozone loss in the high latitude air and the disagreement between model and measurement in low latitude ozone.

(iii) The model overestimate of mixing. The model is probably overestimating the mixing – but then it is being run at 3.75×3.75 degree resolution. However we do not think this

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affects our results as they are closely based on measurements.

We have addressed the specific comments of this referee.

Jens-Uwe Grooss (referee)

1. We have clarified the description of the results. However we prefer to stick with our original strategy rather than the one the referee suggests in his point 1. We think that the revised text makes the rationale for this approach much clearer.

(a) The comparisons of the ozone in SLIMCAT with measurements were pretty good (Sinnhuber et al., 2000). This point has been added to the model description. (b) The differences between the modeled and measured CFC-11 in the polar air are less than 10 ppt, not 20 ppt as JUG states. This is consistent with comparisons we have made of SLIMCAT CFC-11 with that measured by DIRAC on balloons in January (Robinson et al., about to be submitted to ACPD). This paper also compares modeled O_3 with measurements from these flights. We do not refer to this paper since it has not yet been submitted.

2. We have tried to clarify this point. All points shown are either 'pure' model or 'pure' measurement. The model data are output at the time and location of the flight and so are limited in number. We choose not to include more measurements in Figure 4 as we think that confuses the plot. We have improved the text and caption to make things clearer.

3. We agree with JUG that the lowest stratospheric loss should be stated, and we now give the model calculated ozone loss (2–3 DU out of 10 DU) for the 340–400 K partial column in the text of the paper. However we have not changed Figure 5 as we think that information is also useful.

4. The ozone loss tracers shown in Figures 5a and 6a refer to the equivalent latitude at which the ozone loss takes place. We cannot rigorously differentiate between in situ mid-latitude ozone loss and exported polar activation, but we would not expect any

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differences to significantly change the picture shown in these figures.

5. JUG questions the value of including section 5 about the hypothetical flight. This is not a central part of the paper, but we do think that it is an interesting aside and clearly illustrates the evolution of the effect of the polar ozone loss through the first half of the year. The section has been clarified and shortened.

We have addressed all JUG's minor comments.

R. Müller

RM's comment was very constructive. We have included some discussion about the further chemical ozone loss in vortex remnants and now refer to Müller et al.'s and Konopka et al.'s published work on the same winter. The second point made by RM concerns how mixing affected the CFC-11/O_3 relation in June and the implications for ozone loss estimates, and we have included the points that he makes.

The only comment of RM that we do not address in the paper is whether the mid-January reference can be taken as unperturbed. It is not directly relevant to this study which only considers the period from mid-January, and there is some disagreement in the literature which makes it not too simple to raise here.

Interactive comment on Atmos. Chem. Phys. Discuss., 4, 141, 2004.

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