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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.

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This study investigates ozone loss in the lowest stratosphere. It uses aircraft borne observations of CFC-11 and ozone on an Egrett flight on June 5, 2000. These results are combined with a CTM simulation for the same period to investigate the origin of the ozone depleted air and the contribution of the individual ozone loss cycles to the ozone depletion. It is a unique contribution that is worth publishing in ACP. Studies on ozone depletion at these low levels in 2000 has not yet been published to my knowledge, even though there are numerous publications dealing with ozone depletion in this winter. Only the studies of Piani et al. (2002) and Richard et al. (2001) include this altitude range. I suggest revising the paper regarding the points listed below.

General Comment:

The paper starts with a very detailed introduction, description of measurements and model. However, the results are not described in a straight conclusive way. These were clear to me, only after reading this section several times. This could be improved in the revised version.

Specific Comments:

1. The authors base their main results on model passive ozone. It is therefore important to know, how good this quantity is. As the lifetime of ozone in the lower stratosphere is long, the model passive ozone is determined by initialization and transport. Errors in both would propagate into the later results.

   (a) To assure the quality of the ozone initialization, one should compare it with other ozone observations in the lowest stratosphere, e.g. ER-2 in January.

   (b) In figure 4, measured O_3/tracer and modeled passive ozone/tracer correlations are compared to deduce chemical ozone loss. The difference between the data and model can be deduced from figure 3: 200 ppb model CFC-11 corresponds about to 180 ppb measured CFC-11. This distance on the correlation plot is about as large as the deduced ozone depletion.

   Therefore I would propose a slightly different strategy to diagnose chemical ozone loss more that is independent of the model:

   - Show with the model, that the correlation between passive ozone and CFC-11 inside and outside the vortex does (hopefully) not change significantly in the examined altitude range.

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Interactive Comment

- Look at differences between measured correlations only. There are more ER-2 data available in January to construct a reference correlation. Indeed it seems that the correlation would probably be somewhat flater and show larger ozone mixing ratios for high CFC-11 mixing ratios.

2. Did I understand the following correctly? In figure 4a/b, the triangles are pure model O_3/CFC-11 data at the location of the observations. The measured correlations are shown by the circles. As there are very few data points for January dates, I thought first that they are mixtures between model passive ozone and observed CFC-11 that would be hard to interpret. p. 150, l. 28ff could suggest the opposite. But why are only such few data points plotted from the model? Please clarify.

3. This study focuses on data in the lowest stratosphere (370-385K), but often results for higher altitudes are mentioned (e.g. p. 144, l. 13; p. 150,l. 22) or shown (fig. 5, altitude range 342-510K). Especially for figure 5, I would rather like to see how much of this ozone loss is in the lower part of the column (e.g. 340-400K). The paper could be shortened, if this correct information about ozone depletion in higher altitudes would be left out.

4. It is shown that a large fraction of the observed ozone depletion is due to halogen chemistry. But it is not completely clear to me, whether the shown mid-latitude ozone loss at the flight date is due to dilution of ozone depleted vortex air or if active chlorine is transported out of the vortex causing in-situ ozone depletion in mid- latitudes (see p. 142, l. 26ff)

5. What is the sense to discuss a hypothetical flight in section 5? As these results are pure model based, it is not clear to me why the model output is interpolated onto a hypothetical flight path. The point could also been made by displaying the model output in a more general sense.

Minor Comments:

1. The tracer correlation of ozone with CFC-11 is shown. Although the authors state that they did measure CFC-12, H-1211, and CFC-113, they do not show these data. A correlation with these data may be used to clarify whether mixing did play a role. From a nearly linear correlation mixing effects cannot be deduced. Are the other correlations also linear at the flight altitudes?

2. How are the results of the model interpolated onto the flight track? Is the time of the flight track also taken into account or is it only an interpolation of the 12 UT model output onto the flight track coordinates?

3. p. 144, l. 16, remove double sentence

4. p. 149, l. 18. subtropical intrusion "discussed in the previous section". There is no intrusion discussed in section 3.

5. p. 151; fig. 4a: please mention the source of ER-2 CFC-11 and ozone data and acknowledge authors of the data (likely E. Richard for O_3 and J. Elkins for ACATS CFC-11). When using these data, it is required to obey the SOLVE-THESEO2000 data protocol.

6. Are the results found here in line with those of Piani et al.?

7. A study by Konopka et al. (Atmos. Chem. Phys., 3, 839-849, 2003) also investigates ozone loss in vortex remnants and the mixing into mid-latitudes until June. It may be worth mentioning this study, although it concentrates on higher altitudes $\geq$450 K.

8. Reference list: All 11 references to JGR and GRL papers after 2002 are wrong! They should have the proper citation format with doi and citation number, not fake page numbers

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