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Atmospheric Chemistry and Physics Discussions

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Interactive Comment

Interactive comment on "Ozone loss derived from balloon-borne tracer measurements and the SLIMCAT CTM" *by* A. D. Robinson et al.

A. D. Robinson et al.

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1 Anonymous referee #1

This referee has the following concerns: on the measurement consistency throughout the campaign; on establishing a reliable reference O3 / CFC-11 relation at the start of the winter; and on the version of SLIMCAT used to make the model ozone loss estimates. We now address each of these concerns in sections 1.1 to 1.3.

1.1 The ozone loss calculations depend crucially on accurate...

Here referee #1 is concerned that the CFC-11 measurements made above 400 K are biased high and that if the inter-flight bias is not consistent then the ozone loss estimates are questionable. We do not 'hope' that the bias is constant. We make an

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assumption that the bias is constant after consideration of all the available housekeeping data collected from the flights. The DIRAC instrument which collected the CFC-11 data was lost on a balloon flight at the end of the campaign and so thorough post-campaign testing was not possible. However, we did carry out lab tests using our second DIRAC instrument although this was not identical in design to the campaign instrument. These lab tests established a clear, flow dependent, sample carry over effect at the atmospheric pressures corresponding to the 400 to 450 K region. The most important information pertaining to this issue is sample flow rate (this governs the flushing time of the system which is the cause of the bias) and how consistently this changed with altitude between flights (see Fig. 1 given to the editor in file acpd-2004-0164-rp.pdf). At potential temperatures below ˜400 K (˜100 hPa) the sample flow is regulated at ˜22 sccm by an overflow valve. Above this altitude the sample flow decays as the pump is not powerful enough to maintain the desired flow. However, there is a consistent decay in the sample flow rate between flights as a function of potential temperature (similar behaviour is also seen versus atmospheric pressure). This is, in combination with the instrument response curves shown in Fig. 1 of the paper, is the most conclusive evidence we have that the instrument behaved consistently throughout the campaign. We have also added a sentence in the revised manuscript to reinforce our case for consistent instrument performance throughout the campaign.

1.2 On page 7097, line 4 the authors say that a tight correlation...

Referee #1 is concerned that the tight correlation which exists between O3 and CFC-11 at the start of the winter is, in fact, variable within the polar vortex and several profiles may be necessary to establish a reliable reference relation. The referee is making a general point. used a similar tracer relationship approach to determine ozone loss in the 1999/2000 winter. Their early winter correlation was derived from measurements on two well supported balloon flights on 19 Nov and 3 Dec 1999. They argue (and we agree) that a reliable reference relationship was established in early December 1999. We have measurements from two flights in December compared to the one used in

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Muller et al. (2003). Given that we anyway argue that no loss could be observed between early December and late January in our measurements, we do not think any change to the paper is needed on this matter.

1.3 This study uses an old version of the SLIMCAT model...

Referee #1 is concerned here that we used an older version of the SLIMCAT model to make the ozone loss estimates and that this older version performed surprisingly well despite important changes in the new version. We are using this version of SLIMCAT (the only one available to us) because it did reproduce the ozone loss of 1999/2000 rather well (see Sinnhuber et al., 2000) and, in addition, observed ClO in the low stratosphere was also reproduced well (we also validated the ClO from this model extensively against ER-2 measurements with generally good agreement). So we use the model derived ozone loss (which has already been compared successfully to other independent estimates of ozone loss) to compare against our balloon derived estimates. As referee #1 comments, the good SLIMCAT behaviour was in part fortuitous (see below for more discussion on this) but this is not relevant since we are not trying to validate the processes in the model (and any comments which might suggest otherwise have been removed). What's important is the credible ozone loss estimate. However, in respect to the referee, we have added a further caveat about the model performance. Referee #1 points out that there are now versions of SLIMCAT which are improved in several ways. That we obtained a good behaviour in 1999/2000 is in no way contradictory (and certainly does not imply that the new versions do not, for example, represent Cly in the polar vortex well - we don't understand why this comment was made). We had already discussed the performance of the version of SLIMCAT used here over several Arctic winters in Guirlet et al. (2000). That paper showed that in some winters SLIMCAT overestimated the ozone loss and underestimated it in others. It just so happens that 1999/2000 was actually rather good. On the amount of available Cly: We accept the referee's point that there are differences between model and observed CFC-11 which have now led us to qualify the statement about 'generally good agreement' in the first

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paragraph of 4.2. We have also now omitted the first sentence of the second paragraph of 4.2 so as to separate out the model's satisfactory ClO behaviour from any declaration about processes. (We reiterate that the main point is simply that the model reproduces the ozone loss well; we are not trying to argue why the model does well). On the degree of Cl-activation. We already make it clear in this version of the paper that the good performance in terms of ozone loss might have been somewhat fortuitous. Thus in section 4.3 we already say that there is a 'negative temperature bias in the forcing fields' which leads to a good comparison between observed and modelled ClO. Referee #1 (see final three sentences of the review) seems to have completely missed this. Nevertheless, we have added a further caveat. Again, we are not trying to discuss the processes that control ClO. We accept that the modelled ClO above 500K is too high. However, the main emphasis in this paper is at potential temperatures below this level. Following the referee's comments, we have changed the third paragraph in section 4.2 to reflect this.

2 Anonymous referee #2

This referee found the paper excellent, very useful and worthy of publication due to its content in comparing real data with model capacity. The referee had a few specific and technical comments which we now address below. Specific comments: This referee shared concerns with referee #1 regarding the consistency of the CFC-11 measurements from flight to flight. We believe that these issues were dealt with effectively in the response to referee #1 (section 1.1).

Technical issues (note that many of these issues were addressed prior to the paper being accepted for submission to ACPD): Section 2.1: - the phrase "this calibration gas is linked NOAA-CMDL working standards" seems odd. This has now been corrected in the text; Section 2.1: - the authors use a mixture of pg CFC-11 and pg of CFC-11. We now use pg of CFC-11 consistently; Section 2.1: - 3rd paragraph - sentence "Also, laboratory tests at..." massive sentence has been reworded / subdivided; Section 4: 3rd paragraph line 12 "since the O3/CFC-11 was effectively linear". This sentence has

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been changed; Section 4.1: line 10 reference to Fig. 2b added after 3 December; Section 4.2: last paragraph “polar vortex where evidently the model” sentence changed, ‘evidently’ removed; Section 4.3: “evolution has agreed well with data”. ‘has’ now removed;

3 Short comment by B. Vogel

Here the Vogel comments on the significant overestimation in the modelled ClO above 500 K when compared to the measurements made on the HALOZ payload (8 Mar 2000). In Vogel et al. (2003) a similar model ClO overestimation (using the ClaMS model) is reported for a flight made on 1 Mar 2000 using ClO measurements from the TRIPLE payload. Vogel hypothesises that substantial NOx production has occurred at these altitudes by a hitherto unknown mechanism. Whilst these are both very intriguing and independent results we feel it is not within the scope of our paper to come up with a mechanism for the discrepancy. Rather it is sufficient to note that the mechanism remains unexplained and we have now included the Vogel et al. (2003) reference in our paper.

Interactive comment on Atmos. Chem. Phys. Discuss., 4, 7089, 2004.