Atmos. Chem. Phys. Discuss., 4, S3151–S3154, 2004
www.atmos-chem-phys.org/acpd/4/S3151/
European Geosciences Union

Atmospheric Chemistry and Physics Discussions

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

Anonymous Referee #1

Received and published: 29 December 2004

This paper presents the results from a series of "small balloon" launches during winter 1999/2000. The work focusses on the measurements of CFC-11 which are combined with measurements of ozone to determine chemical ozone loss. The results are compared with calculations by the SLIMCAT CTM.

The paper is comprehensive and provides a detailed discussion of the measurements and the model comparisons. But I have considerable problems with some aspects of the paper in its current form. One important issue, the question whether the different measurements are consistent enough to allow quantitative statements, is mentioned in the paper but is not really resolved. Some other important issues are not discussed in the current manuscript. I am afraid that some of the conclusions lack sufficient support from the presented data.

My concerns are mainly based on the following points:

1. The ozone loss calculations depend crucially on accurate measurements of CFC-11 and ozone. But the CFC-11 measurements above about 400 K are obviously biased high. The authors speculate about the source of this problem and they say that one could hope that the bias is constant. But no evidence is presented that this is the case. Without conclusive evidence that the measurements from the different flights are consistent, reliable quantitative ozone loss estimates cannot be based on the data.

2. On page 7097, line 4 the authors say that a tight correlation between ozone and CFC-11 exists at the start of the winter. However, work by Mueller et al. shows that this correlation is indeed quite variable within the polar vortex and a sufficient number of profiles is needed to establish a reliable reference relation. The uncertainty that comes from the variability of the ozone / CFC-11 relation inside the vortex is not included in the uncertainty estimates. The number of profiles in the three ensambles (e.g. only two profiles in the early winter reference) is not sufficient to derive this uncertainty from the scatter of the data.

3. This study uses an old version of the SLIMCAT model. It has been shown that this version of SLIMCAT had significant problems with reproducing Arctic winter ozone loss and a new version was developed in the meantime, which is a tremendeous improvement over the version used here (e.g. Feng et al., submitted to ACP; Chipperfield et al. submitted). At first glance the conclusion of the paper are surprising, because they seem to indicate that the old version did well in some aspects that were changed in the new version:

- the amount of available Cly: On page 7100, line 8 the authors claim that the CFC-11 measurements agree well with the model results and hence, that the amount of available Cly in the model would well represent real atmospheric conditions. However, several changes in the new model formulation increased the amount of Cly inside the

Arctic polar vortex in the lower stratosphere significantly. If published as is, the current paper would imply that the new model does not represent the Cly in the polar vortex well. But I think the conclusion of the current paper is incorrect. In this section it has been neglected that CFC-11 above 400K is probably significantly overestimated by the measurements. This is puzzeling, because this effect has been discussed in some detail in the previous sections of the paper. Why was this not taken into account here ? Also Figure 4 is incorrectly described in the text. Above 400K (including the region 400-450K) the model is quite significantly (up to a factor of two) higher than even these measurements, that have a high bias itself. So CFC-11 in this old model version is probably well too high and hence Cly should be too low.

- the degree of chlorine activation: The discussion of Figure 5 implies that the model does reproduce the processes that control the balance between ClOx and Cly very well. But again a factor of two discrepancy between the model and the measurements (above 500K) is ignored, although this is clearly visible in the Figure. More importantly, it is known that the agreement between modeled and measured ClO at the region 400-500K in March 2000 does not mean that the processes that control ClO are well represented in this old version of SLIMCAT. In winter 1999/2000 the UKMO temperature fields, that were used here, had a cold bias. In the SLIMCAT runs used here this led to the widespread formation of ice particles and to effective denitrification. But in the real atmosphere widespread ice formation was not possible during that winter. Denitrification did nevertheless occurr, but via a mechanism that is not included in the old version of SLIMCAT. Only the new version of SLIMCAT does include the correct process (the formation and sedimentation of "NAT rocks") and does reproduce the degree of denitrification in 1999/2000 when correct temperature fields are used. In the version used here, the correct results (degree of denitrification and hence the degree of chlorine activation in March) are fortuitous and were only obtained because temperatures in the model were colder than in the real atmosphere. I am sure this context is known to the authors. For me it is surprising that this important information is completely ignored in the paper.

ACPD
4, S3151–S3154, 2004

Interactive Comment

Full Screen / Esc
Print Version
Interactive Discussion
Discussion Paper

EGU

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

ACPD

4, S3151–S3154, 2004