Atmos. Chem. Phys. Discuss., 5, S4943–S4946, 2005
www.atmos-chem-phys.org/acpd/5/S4943/
European Geosciences Union
© 2006 Author(s). This work is licensed
under a Creative Commons License.

Atmospheric Chemistry and Physics Discussions

Interactive comment on "On the ability of chemical transport models to simulate the vertical structure of the N_2O, NO_2 and HNO_3 species in the mid-latitude stratosphere" *by* G. Berthet et al.

Anonymous Referee #2

Received and published: 10 January 2006

The paper addresses the ability of chemical transport models (CTMs) to reproduce the vertical structure of N2O, NO2, and HNO3 in October 2002 in the mid-latitude stratosphere by comparing SPIRALE measurements with results from different model runs performed with the 3-D CTM REPROBUS. It contains some important points which will be of interest to the scientific community (although some of the main results are already published by other groups as stated in the manuscript). The paper is well written and I would recommend publication in ACP after some modifications and further investigations.

General Comments:

The scope of the manuscript is the ability of CTMs to reproduce the NOy partitioning in the mid-latitude stratosphere with respect to measurements of the NOy partitioning. The results of the authors are based on the interpretation of one SPIRALE balloon flight performed on October 2nd, 2002, from Aire sur l'Adour in France only. The main conclusion from this single profile investigation is that the modelling of the NOy partitioning relies on a correct simulation of the transport (last sentence of the abstract). To underpin this conclusion and to enhance the significance of the results I would suggest that the authors expand their investigations of the NO2/HNO3 ratio by comparisons at the global scale using satellite data from ODIN or ENVISAT as already mentioned by the authors as next step of their work (see last sentence in the conclusions).

Also, for my point of view the results are somewhat oppositional to the results of Stowasser et al. (2004). Therein, the authors state that "the temporal evolution of the VMRs and of the ratios of the NOy species is fairly independent of the initialization, especially for NO2, ...", which could lead to the conclusion that the X/NOy ratios are nearly independent of the absolute concentration of NOy (and hence of the dynamical effects). This is in contradiction to your conclusions but for me the results of your simulations (see e.g. Figure 10 c) seems to underpin the conclusion of Stowasser et al.. From my point of view, only the NO2/HNO3 ratios of the REPROBUS-ope model run seems to differ substantially from the other model runs which could also be due to differences in the temperature as you do not use the same ECMWF temperature fields to interpolate between. Please specify in more detail why you conclude that the NO2/HNO3 ratio depends strongly from the transport in the model and modify your conclusions accordingly (page 13387 / lines 12 ff.).

To investigate the NOy partitioning in detail (and also to distinguish between chemical and dynamical effects) it is necessary to compare the simulated NOy partitioning as close as possible to the local time and location of the measurement to account for the diurnal variation of the NOy species. In Section 4.1.2 the authors state that the

ACPD

5, S4943–S4946, 2005

model result of the grid point closest to the measurement position has been used to compare with the measurements. What is the distance between the location of the model profile and the measurements and could be simply the distance the reason for the discrepancy between measurements and model calculations as it is shown in Figure 2 that the measurement has been performed in a perturbed meteorological situation? How do the model profiles around the location of the measurement look like? In Section 4.2 the authors "do not consider the diurnal variation of NO2 since it appears to be weak between ... the ascent of the balloon (08:00 UT at about 24 km) and the last measurement (08:30 UT at float altitude).". Could the authors quantify the variation of NO2 between 08:00 UT and 08:30 UT to give the reader an impression of the assumed weak variation with respect to uncertainties of the NO2 measurement (about 7% as stated in Section 2).

The main improvements in the simulation of the N2O profile has been achieved by using 3-hourly ECMWF winds but unfortunately the reasons are still unclear but the authors assume that the 3-h interval is responsible for this improvement (Chapter 5.2, page 12384, lines 18 ff.). Could this improvement in the REPROBUS also achieved by using 6-hourly ECMWF forecasts instead of 6-hourly ECMWF operational analyses (similar to the results of the study of Meijer et al. (GRL, 31, L23114, doi:10.1029/2004GL021158, 2004))?

Specific Comments:

- Abstract / line 13: Please add "of" between "modelling the"

- Section 5.2 / page 12383 / line 8: What is the new N2O profile?

- Section 5.2 / page 12383 / line 22: Why is the discrepancy a limitation of the Michelsen et al. correlation, only? Why it is not related to a limitation of the REPROBUS model?

- Section 6 / page 12385 / line 26: What does "An average of the results ..." mean? How do you average the results?

ACPD

5, S4943–S4946, 2005

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 12373, 2005.

ACPD

5, S4943–S4946, 2005

Interactive Comment