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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Model simulations and aircraft measurements of vertical, seasonal and latitudinal O_3 and CO distributions over Europe" *by* H. Fischer et al.

H. Fischer et al.

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Reply to Referee #2

First we like to thank the referee for his/her helpful comments and remarks. In the following we reply to all comments:

With respect to the additional information provided by our paper relative to the Pfister et al., 2004 paper, we would like to draw the attention of the referee to the final conclusions of the Pfister et al. study. The authors state that there are some essential limitations to the use of satellite CO data for the validation of 3D-CTMs. In particular the MOPITT retrieval provides CO concentrations at seven atmospheric levels, which

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are not entirely independent of each other, thus limiting the significance of the model evaluation with respect to vertical resolution. They conclude that other types of measurements, in particular airborne in-situ measurements are necessary to evaluate the model. The present paper follows this suggestion by using an extensive in-situ data set to evaluate a CTM over a broad range of temporal and spatial ranges. An extension of the study to an investigation of the interannual variability of the CO budgets is a rather extensive enterprise beyond the scope of the present paper and will be addressed in a future publication.

The weakness of the model with respect to the CO budget terms has been addressed in the paper. The major limitation is the use of a climatological biomass burning source for CO, which fails to reproduce the impact of large fires in boreal regions of North America in the summers of 2002 and 2003.

Specific comments:

1. Background CO: We apologize for the confusion. At several locations in the manuscript we used the wrong phrase “background CO” instead of the correct expression “photochemically produced CO”. In the revised version we changed to emphasize that we calculate a photochemical CO source that is not identical with the CO background, which is indeed a combination of primary emissions, long range transport and photochemical production.

2. Latitudinal dependency of STE overestimation: The statement that the model in general tends to overestimate stratospheric ozone at low latitudes and underestimates it at high latitudes is indeed misleading. In general the agreement between the model and the observations for stratospheric ozone is good at all seasons and latitudes. Exceptions are overestimations of stratospheric ozone at low latitudes in spring and fall, and underestimations of stratospheric ozone at high latitudes in spring and summer. This is surely not a general trend, and most probably due to the mentioned misrepresentations of the tropopause height in the model due to the coarse resolution and the

ACPD

5, S4581–S4584, 2005

associated vertical displacement of the stratospheric ozone profile. We changed the text in the manuscript in the following way: The model overestimates stratospheric O3 mixing ratios at low latitudes during the spring and fall seasons (Fig. 2a and g) and underestimates it at high latitudes during spring and summer (Fig. 2c and f).

3. Isentropic transport in MATCH: The SPURT data indicate maximum O3 concentrations in the subtropical UT in summer, which is in agreement with the analysis of Jing et al. based on SAGE II ozone measurements and ozonesonde data. This O3 maximum is not reproduced by MATCH, which is either due to an underestimation of the transport of O3 from the stratosphere (either diabatic or quasi-isentropic) or due to an underestimation of photochemically produced O3. The study of Jing et al. identifies isentropic cross-tropopause transport as the prime source of enhanced O3 in the UT. Due to the coarse resolution of an Eulerian model like MATCH it is likely that we would underestimate such a process. But at this stage we can't test whether the hypothesis by Jing et al. is correct and therefore we change the text in the following way: Therefore, the discrepancy can be either due to an underestimation of transport of O3 from the stratosphere (e.g. due to an underestimation of isentropic transport across the subtropical jet as suggested by Jing et al., 2005), or an underestimation of the net O3 production in the free troposphere by the model.

4. Inconsistency of numbers for O3 at 8.5 km: We double checked the numbers cited in the text and those used in figure 2, and there is no disagreement.

5. Oxidation of CH4: Due to its long photochemical lifetime of the order of 10 years CH4 is well mixed throughout the troposphere. The CO source resulting from the CH4 oxidation is thus largely independent of the geographical origin of the methane, since local variations are quite small (i.e. less than a few percent of the tropospheric mixing ratio).

Technical comments:

The typos were corrected and the text was modified along the lines suggested by the

ACPD

5, S4581–S4584, 2005

referee. In particular Figures 2 and 3 were revised to improve their readability.

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 9065, 2005.