

Interactive comment on “Model simulations and aircraft measurements of vertical, seasonal and latitudinal O₃ and CO distributions over Europe” by H. Fischer et al.

Anonymous Referee #2

Received and published: 23 November 2005

On the whole, the paper is well written, well organised and has clear conclusions. The paper analysed the performance of the MATCH model in relation to the SPURT profiles and identified a number of model weaknesses. The study also included the tagging of CO sources to assess the CO budget over Europe. Although it is reassuring for MATCH to be in quantitative agreement with the CO budget study by Pfister et al. 2004, I felt as the paper stands, it did not provide any further insight. Given that MATCH was run over a number of years, perhaps the authors could examine the interannual variability of the CO budget - something which was not explored in the the Pfister et al paper. To what extent can the authors comment on the impact of the model weaknesses on the CO

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budget terms? If the authors can address the above issues, then I would recommend publication in ACP.

In addition, I have a number of specific and technical comments as follows:

Specific Comments

1. The background CO is also calculated as two separate tracers from the photochemical oxidation of CH₄ and VOCs. This comment is not very clear. Surely these two tracers contribute to the "background" CO in addition to the primary CO emissions.
2. Section 4.1 In general the model tends to overestimate stratospheric O₃ mixing ratios at low latitudes, underestimates it at high latitudes, while the agreement is best at mid-latitudes. This is most probably due to the coarse resolution of the model around the tropopause... Why should this affect tropics and high-latitude comparisons more so than mid-latitudes?
3. Section 4.1 You say that Jing et al found there was enhanced isentropic transport during summer, resulting in an O₃ maximum in the UT of the subtropics, which is in agreement with our findings. Can you be more explicit here? Are you saying that the SPURT observations are in agreement with Jing et al. and that MATCH fails to capture that isentropic transport?
4. Section 4.1 You give comparisons of numbers for observed and modelled O₃ mixing ratios at 8.5 km. However, for the low and high latitudes, they do not appear to be consistent with Figure 2.
5. Section 5 The largest contribution to the CO profiles is due to the oxidation of CH₄. Can you comment on the geographical origin of the methane?

Technical Comments

1. Section 3 Simulations ... nitrogen compounds and methane are tied to observations from the HALOE (not HALO) project ...

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2. Section 4.1 between 300 and 200 hPa (and not hPA)
 3. Section 4.2 by Brunner et al. (2003) in an evaluation of five global CTMs.
 4. Figures 2 and 3 Can you improve the clarity of these plots, either by increasing the thickness of the lines or the size of the plots, or by choosing other colours (in the case of Figure 3)?
 5. Table 2 The mean observed CO concentration at 5.5km at low latitudes appears to be incorrect.
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Interactive comment on Atmos. Chem. Phys. Discuss., 5, 9065, 2005.

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