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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Influence of convective transport on tropospheric ozone andits precursors in a chemistry-climate model" *by* R. M. Doherty et al.

R. M. Doherty et al.

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Here is our final response. All comments are addressed. The referee's original comments are in italics.

Responses to referee 2.

Specific comments: Section 1. The authors mention that lightning NOx play an important role by reacting with isoprene in the UT. However, these emissions are still highly uncertain. Could the authors mention and discuss the total lightning NOx emissions in their model? Could they compare their number with that of the previous studies they refer to?

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Global lightning NOx emissions are 7 TgN/yr. We have added this text to Section 2. We have clarified our text comparing the global total magnitudes used in the two studies in the discussion (page 3759):

“A comparison of the emissions used in the two studies shows that NO surface, aircraft and ship emissions are similar but lightning NOx emissions are higher at 7 TgN/yr in our study compared to 4.9 TgN/yr in Lawrence et al., 2003)”

Section 2. Could the authors be more precise on the performances of their model, especially of the convective scheme? They mention that experiments were performed with radon, however they should provide more details on this evaluation. They could also try to discuss their model’s performances in terms of convective precipitations, for example.

We have expanded text in section 2 to include a synopsis of the results of the evaluation of simulated and observed radon profiles performed by Collins et al., (2002). For precipitation see response to referee ML, comment 4. We have compared precipitation from our model with that of the GPCP climatology (see referee ML comment 4). We have also revised Figure 1 and compared HadAM3 convective mass fluxes with ERA-40 convective mass fluxes. This text now forms a substantial section in the discussion. (See response to referee ML comment 4 for text).

Section 3.1. The discussion in that section is somewhat hard to follow, in part because a number of processes are involved in the budget of each region. Could the authors synthesise their results in a table or a "cartoon" which would include the budget (i.e., transport and chemical terms for key reactions associated with ozone and NOx production and loss) for the different regions they consider in both the control and no-convection simulations? One of the rationales for conducting this study is to gain understanding how future climate may affect tropospheric chemistry. Thus, if possible,could the authors mention the effect of convection on the global OH budget? In Table 1, it is seen that the NOx burden changes substantially between the two runs.

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What about the NOx lifetime?

We have added a summary table of the importance processes in section 3.1 (now section 3.2) and their direction of change as suggested by referee 1 comment 6. We can only compute budget terms globally. We do not have the transport terms - inflow and outflow fluxes for each region that we would need to calculate regional budgets.

We have included changes in OH and the NOx lifetime in Table 1. The revised Table can be seen at http://www.met.ed.ac.uk/~dstevens/convection_paper/tables.doc. The effect of convection is to increase the OH burden by 8%. We have now calculated the NOx lifetime. This decreases from 1.4 days (convection off) to 1.1 days with convective mixing. We have added text regarding these results to the discussion of Table 1 in section 3.4.

Section 4. The authors state that the largest difference between their study and that of Lawrence et al. (2003) may be the convective schemes. I think the paper would benefit from a more detailed discussion on the main differences between these schemes. Are the schemes fundamentally different and in which manner (i.e. location, strength, etc.)? Was the experiment of Lawrence et al. (2003) conduct in a similar manner (i.e. with water vapour and lightning NOx kept constant)?

We have performed a comparison of the Lawrence et al and our convective mass fluxes and have added substantial text to section 4. See response to referee ML comment 4 for details and text. Figure 8 (http://www.met.ed.ac.uk/~dstevens/convection_paper/figure8.pdf) compares the updraught fluxes in the two studies. The updraught fluxes from the two schemes do vary substantially in height and strength.

Water vapour and lightning NOx were kept constant in Lawrence et al. (2003) (see referee ML comment 18). We have included this text in the discussion in section 4.

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 3747, 2005.

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