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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 4.

The differences (and the causes of these differences) between this study and that of Lawrence et al. should be addressed in more detail in this paper.

We have added substantial further discussion to section 4, with regard to model differences, particularly convective mass fluxes. See response to referee ML comment 4 for text and details.

It seems to me that the paper would strongly benefit from inclusion of some comparisons with obser vations. How well does the model convective parameterization represent convection in the tropics and midlatitudes? How well does the model replicate observations of NOx and O3 in the middle and upper troposphere?

We have added a new section 3.1 entitled "Evaluation of modelled NOX, PAN and O3 against observations." This text is included in the response to referee ML, comment 1. There is a tendency to underestimate MT and UT ozone in the tropics; however model results are within one standard deviation of observations.

Specific Comments: Abstract: The statement that "Convective lofting of NOx from surface sources appears relatively unimportant" leads me to question the overall results of this research. This is contrary to much previous research on the subject. I suspect that this result is the primary reason that the effect on global tropospheric ozone in this study differs significantly from that in the Lawrence et al. work.

This statement applies only to the tropics where lightning NOx emissions are of similar magnitude to surface sources. Text is clarified.

The abstract ends with a statement saying, "Further modeling studies are needed to constrain the uncertainty range". I think that what is needed first and foremost is further evaluation of convective parameterizations. I strongly suspect that substantial differences in the characteristics of the convection (location, frequency, updraft strength, etc.) are the root cause of the difference in the net effect on ozone between this work and that of Lawrence et al.

The convective updraught fluxes in the two studies have been compared in Figure 8 (http://www.met.ed.ac.uk/~dstevens/convection_paper/figure8.pdf), and substantial text has been added to the discussion in section 4. There are large differences in the height and strength of deep convection, and these may well explain much of the differences in the impact of convective mixing on tropospheric ozone. See response to referee ML comment 4 for revised text. The HadAM3 convective mass fluxes are compared to convective mass fluxes from ERA-40 renalyses in a revised figure 1 (http://www.met.ed.ac.uk/~dstevens/convection_paper/figure1new.pdf). Again see response to referee ML, comment 4 for text.

Introduction - first paragraph - This is written as if the concept of convective trans- por t of trace gases was fairly new. The introduction would benefit by containing more background material on convective transport and its consequences for atmospheric chemistry. It should reference some of the earlier measurement and modeling work (late 1980s and early to mid 1990s).

We have added references to some of the earlier work.

Introduction - second paragraph - This paragraph only addresses natural sources of NOx and NMHC. What about the impact of convective transport of anthropogenic emissions? At least some mention of this process should be made. The Pickering et al. (1998) profiles provide the vertical effective source distribution for lightning NO (including the production and transport within the convection that produced it). Subsequent convection could redistribute these emissions either upward or downward. Please clarify this within this paragraph.

We have added text regarding convective transport of anthropogenic emissions.

Yes, in our model set-up we use the Pickering et al., vertical profiles to distribute lightning NOx emissions, and these then enter into the model transport and mixing schemes. We have added text to section 2 to clarify this. We have removed the sentence regarding the impact of convection on lightning NOx as it is misleading.

Section 2 - 2nd and 3rd paragraphs - Has the STOCHEM model been extensively evaluated against trace gas observations? Collins et al. (2002) performed an evaluation with radon, but has it been rigorously compared with ozone and precursor observa-

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tions? If not, that type of analysis should be included here.

We have included a synopsis of the radon evaluations performed by Collins et al., (2002) in section 2. We have added a new section 3.1 that evaluates modelled NOx, PAN and O3 against aircraft observations from Emmons et al., (2000). Text for section 3.1 is given in the response to referee ML, comment 1.

page 3751, lines 1-3: Please compare this 20:1 land to ocean ratio with the sstellite lightning climatology of Christian et al. (2003, JGR). Globally, the flash ratio is about 10:1. The Price et al. lightning scheme tends to generate too few flashes over the oceans. What does the global distribution of lightning NO emissions look like? What is the total global production of lightning NO (in terms of Tg N/yr)?

We thank the referee for this interesting reference. We use the Price et al lightning NOx scheme as is used in a number of other tropospheric chemistry models (e.g see Brunner et al., 2003 ACP 3, 1609-31). We have re-examined the ratio of tropical UT land to ocean lightning NO emissions. The value 20:1 was erroneous; it should be 40:1! The global annual average ratio is 30:1. Note however that NO emissions are not directly proportional to the total number of flashes, because cloud-to-ground and inter-cloud flashes produce different amounts of NOx. Thus, we cannot directly compare the ratio of land-to-ocean flashes with the ratio of land-to ocean NO production. Unfortunately, we do not have the number of flashes archived to make the direct comparison. We see that Labrador et al., (2005) compare their flash rate which is calculated from Price and Rind against that of Christian et al., (2003). They also find a lower flash rate over the oceans compared to that of Christian et al., (2003). The global distribution of lightning NO emissions from HadAM3-STOCHEM can be seen at http:www.met.ed.ac.uk/ dstevens/convection$_{paper/lnox.docItisfairlysimilartotheflashratedistributioninFigure4ofChr}$
$tropicaloceaniclightningappearslowerandlightninginthesouthernNorthAmericanandcentralNorthernEurasiancontinen$

Section 3 - Results Â Figure 1. This distribution of convection should be compared with a satellite-based climatology. There should be frequent convection reaching to near the

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tropopause over North America especially in the summer. I suspect that midlatitude convection is underrepresented in this plot

We have compared precipitation against the GPCP satellite climatology and it compares favourably (see http://www.met.ed.ac.uk/~dstevens/convection_paper/compareprecip.doc). We have also compared our model convective updraught fluxes with ERA-40 updraught fluxes in a revised Figure 1 (http://www.met.ed.ac.uk/~dstevens/convection_paper/figure1new.pdf). See response to referee ML comment 4 for further details and revised text. Figure 1 does show convection at 200hPa over parts of North America.

Section 3.2 - Here again the midlatitude convection is referred to as "relatively shallow". Midlatitude deep convection appears to be lacking in this model. I think this is the main reason for the large difference in the results between this study and that of Lawrence et al. (2003). I would suggest actually comparing the convection in your model versus that from MATCH-MPIC. This comparison will likely explain a large portion of the difference in the ozone results. Then, some determination should be made as to which set of parameterized convection is closer to the truth.

This statement is misleading and we have revised text in section 3.2. We have performed a comparison of convective updraught fluxes simulated in our study with that of Lawrence et al.,(2003) in figure 8 (http://www.met.ed.ac.uk/~dstevens/convection_paper/figure8.pdf). See response to ML comment 4 for added text to the discussion in section 4. There are large differences in the strength and height of convective mass fluxes between the two models. See response to referee ML, comment 4 for text.

p. 3760, lines 23-28: If the convection between the two models is actually compared, then this paragraph could be strengthened from just speculation to some actual knowledge of the characteristics of the convection in the two models and the impact that the *differences have on transport and subsequent chemistry.*

We have performed this comparison and included substantial text in section 4, as discussed above.

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

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