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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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We would like to thank all the referees for their detailed and thoughtful comments on the paper. Here we present our initial responses to most of the referees' comments. Not all points raised by the referees are dealt with here. We will include a full response to all comments in our final response.

Some responses to referee 4:

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

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observations of NOx and O3 in the middle and upper troposphere?

We will add a comparison with observations section after section2, and discuss previous work on comparison of the model with observations. See ML comments 1 and 4, and referee 1 comment 3, referee 2, comment 2.

Abstract: convection 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. 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.

We hope to compare convective mass fluxes between the two studies (see referee ML comment 4, referee 2 last comment) and will add a discussion of this to the text.

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 will add text regarding the impact of convective transport of anthropogenic emissions. 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 will add text to clarify this.

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

Model evaluation for ozone with the current convection scheme was performed in Stevenson et al., (2004) and Dentener et al., (2005). These results were very briefly mentioned in section 4 (p. 3760). We will add a new section after section 2 to expand on this text and perform a NOx and PAN evaluation, see ML comment 1 and referee 1 comment 3, referee 2 comment 2.

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 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 is 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, as the 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 land-to ocean NO production. We do not have the number of flashes archived to make the direct comparison. The global distribution of lightning NO emissions (which cannot be displayed in this on-line comment) is fairly similar to Figure 4 of Christian et al., (2003) and the global total is 7 Tg N/yr. We will add text on this.

Section 3 - Results Âň Figure 1. This distribution of convection should be compared

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with a satellite-based climatology. There should be frequent convection reaching to near the tropopause over Nor th America especially in the summer. I suspect that midlatitude convection is underrepresented in this plot.

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.

p. 3760, lines 23-28: If the convection between the two models is actually compared, then this parargraph 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 compared convective precipitation with the GPCP satellite climatology, see referee ML comment 4. In order to compare differences in convection height (i.e. shallow and deep convection) we intend to compare our convective mass fluxes with those of Lawrence et al (2003). We will then add text to the discussion and any other relevant sections.

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