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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.

Anonymous Referee #4

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General Comments: This manuscript provides a detailed account of the effects of deep convection on tropospheric ozone and its precursors based on a Lagrangian chemical transport model. The global result of including convection in the model was a 15% decrease in the tropospheric ozone burden. This result is in contrast to that of Lawrence et al. (2003) who found that convection causes a global increase of tropospheric ozone. 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. It seems to me that the paper would strongly benefit from inclusion of some comparisons with observations. 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? There are a multitude of observations (sondes, aircraft, satellite) which could be used to evaluate the model. I would recommend that the paper be published contingent on addressing these major deficiencies which are further elaborated upon below.

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.

Introduction - first paragraph - This is written as if the concept of convective transport 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).

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.

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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.

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)?

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

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