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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 #1

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Review of "Influence of convective transport on tropospheric ozone and its precursors in a chemistry-climate model" by Doherty et al. This paper gives a nice explication of the various ways in which convection affects the ozone budget including both chemical and transport effects. As the paper points out, understanding these factors are important in predicting future changes in atmospheric composition. However, there are a number of serious issues which I feel need to be addressed before the paper can be published:

Specific Comments:

1) I really don't understand the global ozone budget with and without convection. The tendency equation for ozone consists of transport, production and loss. In a global integral the transport of ozone cancels out (due to mass balance considerations). The result is the globally integrated ozone can only change through net ozone production (neglecting dry deposition). Global net ozone production increases in the author's model, but global ozone decreases. The authors attribute this inconsistency to the fact the ozone lifetime decreases (e.g., page 3759). I do not think this explanation is correct. The problem is the lifetime (defined, I presume as the inverse of the ozone loss) does not take the production of ozone into account. In the author's analysis the production and loss increase with convection, but the production increases more. Thus, ozone should increase even though the lifetime decreases. This suggests that either: a) there is some problem in the analysis, or b) transport does not cancel because their global budget is not really global (e.g., the budget is over the troposphere so that the ozone decrease is due to differences in the stratosphere-troposphere exchange). If the result can be attributed to differences in stratosphere-troposphere exchange, this suggests a large sensitivity to convection. It is not at all clear to me that a 9-level model can accurately simulate this sensitivity. 2) I am somewhat surprised at Figure 6a and the author's interpretation. If I understand the authors correctly they interpret the convective mid-latitude upper troposphere and lower stratosphere ozone decrease as a global propagation of the tropical signal. The author's make a heuristic argument for this based on the height of convection, but do not show more definitely that the signal propagates from the tropics to the mid-latitudes. In fact there are some reasons to suspect it shouldn't: both Bowman and Carrie [2002] and Pierrehumbert and Yang [1993] suggest a barrier to mixing between the tropics and extra-tropics. The standard wisdom is that significant upper tropospheric transport across the subtropical jet only occurs in the N.H. in association with the summer monsoon (e.g., Chen, 1995). Yet Figure 6 also shows strong transport to the Southern Hemispere. Could the authors give more conclusive evidence that the signal they show is indeed due to a poleward propagation of the tropical signal? 3) I'm somewhat disturbed by the lack of observa-

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tions in this paper. The authors should show, or give references to the fact that the model does a reasonable job at simulating upper tropospheric ozone. How well does the model predict precipitation? 4) The authors made extended simulations (20 years) with convection on and off. This should allow them to include the interannual variability in Table 1. They also should be able to make an assessment of whether the differences between the two cases are significant or not. 5) The authors show qualitatively that convection decreases upper tropospheric NOx through conversion to PAN. Can the authors make this argument more quantitative: what fraction of the NOx decrease can be explained by the PAN increase? 6) The authors have identified a number of competing factors whereby convection influences ozone: transport changes both the ozone and NOx concentrations; chemistry decreases ozone production through PAN production, but increases the production through modifying the availability of HO2. The net result is obviously dependent on the cancellation of a number of processes. Barrring a more quantitative comparison, it would be helpful to make a table of these various processes (e.g., what processes are increasing ozone/decreasing ozone) and to indicate which the dominant processes are. 7) Discussion of the N.H. convective influence (page 3756). It is not clear to me if the authors have made any additional calculations to ensure that their explanations are indeed correct? For example the annual importance of a convective influence extending up to 500 mbar during winter seems doubtful to me. Another explanation might be that summertime convection evacuates the boundary layer of NOx, an influence which is transported northwards in warm conveyor belts. The authors need to make clear if their explanations are fully backed up by further analysis, or if they are simply hypotheses. 8) While the authors extensively discuss on the interplay between convection and chemistry in the tropics, they do not really touch on this subject in the mid-latitudes. One might think that transporting NOx to the middle and upper troposphere would increase ozone net production. Could the authors comment a bit more on the importance of chemistry in the mid-latidudes (it seems to be mentioned in passing on the bottom of 3757, the top of 3758)?

Technical Comments 1) I could not find information on the global production of NOx from lightning. 2) Due to the importance of the convective scheme in this paper, it would be appropriate if the author's could briefly describe its characteristics. For example, does the convection scheme include downdrafts? 3) How is ozone production and loss computed? These quantities depend on the definition of odd oxygen.

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