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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Evaluation of near-tropopause ozone distributions in the Global Modeling Initiative combined stratosphere/troposphere model with ozonesondedata" *by* D. B. Considine et al.

Anonymous Referee #5

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1 General comments

The paper presents an evaluation of the NASA GMI CTM *Combo* driven by the GMAO GEOS4 General Circulation Model using ozone sondes. It is focussing on multiyear monthly averages using coordinates relative to the tropopause in observations and simulation. In contrast to the evaluation of the same model in Strahan et al. (2007), which used satellite and aircraft campaign data, with the ozone sondes the vertical res-

olution of 1km of the model near the tropopause appears to be not sufficient, findings which are consistent with other recent model evaluation papers in ACP. The paper is worth to be published but there is a need to analyse in more detail the effect of interannual variability, especially in high and midlatitudes of the Northern Hemisphere. In the paper 16 years of observations are compared with only 5 years of simulations using an almost free running GCM only constrained by sea surface temperatures, a timeseries which is too short with respect to the interannual variability of the Arctic vortex. Also, in the observations (e.g. Hohenpeissenberg, Steinbrecht et al. (1998)) a trend in the tropopause height is present which cannot be significant in a 5 years simulation.

It would be useful to carry out simulations with the CTM driven by meteorological analysis and do a point by point comparison with the ozone sonde data for better separation of chemical and dynamical effects. Has this been done or are there plans to do that?

2 Specific Comments

2.1 Model description

Isoprene oxydation should not have an impact near the tropopause. If it is mentioned also the approach or a reference should be given. Am I right that the lightning parameterization is independent of the convection in the underlying GCM? That could lead to inconsistencies in ozone.

2.2 Results

4.1. A figure with the interannual variability of total ozone in model and observations might be useful. Concerning Figure 3 it is not honest to cut the regions in high latitudes

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where the model ozone in the middle stratosphere can be low by more than 40%, indicating a problem with the circulation (figure was complete for the access review). If not shown, there should be a remark in the text.

4.3. Only 5 datapoints per month are not significant for regression. What can be learned if the regression is done for all months in one figure in the presence of large seasonal differences? Concerning STE there are several more recent references. What latitude range is included for extratropical ozone? Is it fixed or determined by dynamics?

4.6. The last sentence appears to be in contradiction to the shown figures.

3 Technical Comments

In Table 1 or 2 should be indicated which stations are used in Figs. 15 and 17.

Is in Fig. 12 the same set of stations as in Fig. 11? The caption is inconsistent with the labels in the figure and might be shortened. Also bars indicating the variability would be useful.

In Fig. 17 a frame is missing, the labels should be as in Fig. 15.

On top of page 1603 and in the Considine 2005 reference are typos.

4 References

Steinbrecht, W., H. Claude, U. Köhler, and K. P. Hoinka (1998), Correlations between tropopause height and total ozone: Implications for long-term changes, J. Geophys. Res., 103(D15), 19,183-19,192.

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Interactive Comment