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Interactive Comment

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.

D. B. Considine et al.

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General Comments:

We would like to thank the referee for the time spent reviewing this paper. The comments of all the referees have been very useful, and have helped us to produce an improved revised version which will be submitted to the journal shortly.

We have responded to most of the comments and suggestions included in the reviews, though some of the suggested modifications and additions were infeasible at this time. Below we list in italics the comments of Referee 3 to which we have responded, followed by our response in regular font.

Specific Comments:

"Due to the vertical ozone gradient at the tropopause one has to consider that the model, even if it was perfect, will reproduce an average ozone over a 1 km altitude range....Therefore the data used in the comparison ... must not be interpolated to the altitude but averaged over the vertical grid interval). Maybe it is done in that way already, then please mention it clearly."

This is how the pressure-averaged ozonesonde profiles were constructed. This is spelled out in section 2: "...the pressure levels were changed from irregular intervals (1000, 900, 800 hPa etc.) to 35 levels equally spaced in pressure altitude between 1000 and 5 hPa ($\sim$ 1 km apart), and averages were formed for each pressure level, with interpolation used only if there were no measurements in a layer." The RTT-average climatology, on the other hand, was constructed using interpolation in a manner which would raise the concerns expressed by the referee. However, we do not think that our method is a problem. First, to be clear, the problem only arises if the ozone concentration from the bottom to top of a level increases nonlinearly. If it increases rapidly but linearly, the layer average and the central value of the layer will be equivalent. Second, the degree of the problem depends on the resolution difference between the sondes and the model. This turns out to be not that large. The effective resolution of the sondes is about 300 meters due to the time it takes to pump air through the solution, even though sondes are reported at a finer vertical spacing. This means that each model layer will contain at most 3 or 4 sonde values for a given profile. This is not sufficient to resolve very well nonlinear changes in concentration a level, and suggests that the difference between interpolation and averaging will be small. Last, we conducted a test to evaluate the potential error, and found it to be small. First, we generated pseudo high resolution profiles with 4 points/km from the RTT climatology, using cubic spline interpolation to ensure nonlinear changes within each level. Then we layer-averaged these, and compared to the RTT profiles. We found differences on the order of 1% -

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3% at the tropopause - not enough to be a concern. We have revised the discussion in the paper to make our methods clearer.

"The issues of vertical diffusion and flux at the tropopause would be better addressed if the vertical resolution rather than the horizontal resolution is increased."

As we have explained in our response to Referee 1, increasing vertical resolution requires driving the CTM using a meteorological data set from a high-vertical resolution GCM or DAS. There is no higher resolution data set currently available for the Combo CTM, so increasing vertical resolution is infeasible at present.

"The interpretation of the differences between observations and the simulation is not clear: The authors suggest insufficient vertical resolution or too high vertical diffusivity for as reason for the ozone high bias (p. 1608/l. 25). Why would then the discrepancy increase for the lower model resolution?"

Doubling the horizontal resolution decreases the horizontal diffusivity of the model, reducing tropical ozone values and increasing tropopause ozone values in the extratropics. This results in a decreased tropical and increased extratropical high bias, but the high bias is not eliminated. We then examined the possibility of reducing STE to eliminate the bias, and found that would require unrealistically large reductions in STE. We conclude on the basis of this analysis that it is more likely that excessive vertical diffusion or insufficient vertical resolution is the cause of the high biases. However, we are unable to explicitly test this conclusion at the present time due to the lack of suitable higher-resolution meteorological data.

"The comparison of tropopause ozone between model and observations (figures 5-7) is of course a challenge to the model, since both the tropopause height and the ozone profile with a strong gradient must be simulated correctly. The comparison of the whole profile (figures 9-12) is a better way to judge the model performance."

We agree that both tropopause ozone and the ozone profile comparisons are worthwhile, which is why both are included in this paper. Referee 2 also made this point. We feel that it is worth presenting comparisons at the tropopause, but the paper does not focus entirely on tropopause ozone values.

"How is the tropopause determined to a finer resolution than given in the model run (e.g. in Figure 4)?"

To calculate model tropopause height we first interpolate profiles to a 0.1 km vertical grid using cubic spline interpolation prior to identifying the thermal tropopause height using the WMO definition. We experimented with linear interpolation but found little difference in our results. We have revised the paper to mention this. Our test of sensitivity to tropopause height consisted of examining model ozone at observed rather than model predicted tropopause heights, presented in Figures 5 and 6.

"Figure 2: It would be nice to see a difference panel similarly as shown in fig 3."

This figure has been modified in the revised version.

"Figure 3, lowest panel: name the values of the shown contour levels in the caption. Probably it is 0, $\pm$10%, $\pm$20% . . . "

This has been added to the caption.

"Are the comparisons in figs 4-6 similar in the Southern Hemisphere midlatitudes?"

We have no sonde data in the climatology from the SH midlatitudes with which to compare the model results.

"Page 1599/Figure 8: Explain briefly, how the STE or the monthly mean NH extratropical cross tropopause O3 flux is determined from the model."

The flux of O_3 across the tropopause is calculated as the difference between its flux across the 380 K surface and the change in O_3 in the lowermost stratosphere. We have included a sentence of explanation in the revised manuscript.

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"Figure 9 shows data for Januaries between 1985 and 2000. Page 1600, line 5 refers to only 49 sonde profiles. This is probably a typo. Figure 10 shows the corresponding model values, but for 155 profiles between 1994 and 1998. What is the reason for the different time range (and and different number of profiles)"

There are only 49 sonde profiles in the sonde climatologies for Edmonton in January, betweeen 1985 and 2000. This is basically due to the frequency with which sondes were launched at Edmonton in January between these dates. We chose the 1985-2000 time period to provide sufficient profiles to provide reasonable statistics, and to approximately overlap the 1994-1998 model run period. We have 155 daily model profiles because the model simulation was run for 5 years. This is currently the longest meteorological data set available for Combo model simulations. The revised version of the paper outlines the effects of the differing observational and model statistics in its discussion of Fig. 4, which points out that the model standard error bars are smaller than observed standard error bars due to the higher number of daily profiles, rather than differences in model and observed variability.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 1589, 2008.