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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 #3**

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## General

The paper focuses on the validation of the COMBO model, a CTM that covers both the troposphere and the stratosphere using ozone sonde data.

Two methods of averaging are considered and compared, averaging on pressure surfaces and on pressure altitude relative to the tropopause.

The analyzed value, ozone mixing ratio at the thermal tropopause, is difficult to determine, firstly because of the large and changing vertical gradient in ozone mixing ratio and secondly because of the exact determination of the height of the thermal tropopause due to model vertical resolution at the tropopause.

One weak point seems to me the interpretation of the vertical resolution. It is said that the vertical resolution is about 1 km at the tropopause height for both considered horizontal resolutions.

The paper is well written and should be published after the points below have been sorted out.

## Major issues

1. 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. The nature of the ozone profile, well below 100 ppb in the troposphere and well above in the stratosphere would result in a larger tropopause mixing ratio if averaged over a 1 km vertical range around the tropopause. Therefore the data used in the comparison must be degraded to the model resolution (i.e. it 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.

2. 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.

3. 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? Possible under-estimation of the impact of biomass burning on the ozone production are mentioned, but it



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is not clearly explained how the authors come to this assumption.

## Minor issues

1. 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.
2. How is the tropopause determined to a finer resolution than given in the model run (e.g. in Figure 4)?
3. Figure 2: It would be nice to see a difference panel similarly as shown in fig 3.
4. Figure 3, lowest panel: name the values of the shown contour levels in the caption. Probably it is 0, $\pm$10%, $\pm$20% ...
5. Are the comparisons in figs 4-6 similar in the Southern Hemisphere mid-latitudes?
6. page 1599/Figure 8: Explain briefly, how the STE or the monthly mean NH extra-tropical cross tropopause $O_3$ flux is determined from the model.
7. 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).

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



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