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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.

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 5 to which we have responded, fol-

lowed by our response in regular font.

Specific Comments:

"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."

Our goal in this paper was to evaluate the model ozone near the tropopause using ozonesonde observations. Because a GCM-based meteorology is used to drive the model, we cannot expect to reproduce any particular sonde and so develop a climatological evaluation based on comparing monthly averaged model and observed profiles. Because our model was only integrated for five years, we have also intentionally not focused on whether the simulation shows any trends over those years. That said, we do examine or at least compare modeled and observed variability by including standard error bars in Figs. 4, 5, 6, and 11. In addition, the importance of variability is explicitly discussed when presenting the differences between pressure and RTT averaging in Figs. 9 and 10. In making these comparisons, we did not explicitly state that the standard deviations calculated from 5 years of data are good approximations to the long-term values. We checked this by calculating the standard deviations using subsets of the data, and found that the standard deviations converge after only 2 years, even at high northern latitudes. Thus, a 5-year data set is sufficient to ensure good estimate of long term model variability. We now include a statement to this effect in the paper.

"It would be useful to carry out simulations with the CTM driven by meteorological

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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?"

This has not been done. While it would be interesting, such a study is essentially different from the one we are describing in this paper. First, the comparison strategies would be different, due to the fact that individual sondes are in principle comparable to daily model ozone profiles. Second, it is well-known that vertical wind fields in CTMs driven by analyzed meteorological data are very noisy, a result of the insertion of observations into the running GCM upon which the analysis is based. The focus of a study including simulations driven by both GCM and DAS-based meteorological data would thus be on the extent to which the noise caused by data insertion influences the modeled vertical ozone profiles in the DAS-based simulation. The current study focuses on comparison methodologies and the degree to which a GCM-based simulation can reproduce vertical ozone profiles near the tropopause, without the added complication of data insertion.

"Isoprene oxydation should not have an impact near the tropopause. If it is mentioned also the approach or a reference should be given."

We now provide a reference in the revised version.

"Am I right that the lightning parameterization is independent of the convection in the underlying GCM? That could lead to inconsistencies in ozone."

You are right, the horizontal distribution of lightning emissions is based on the ISCCP deep convective cloud climatology. This provides a climatologically correct emissions distribution, and we do not feel that it could be an issue for this paper.

"Concerning Figure 3 it is not honest to cut the regions in high latitudes 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,

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there should be a remark in the text."

The latitude range of this figure was cut because it was correctly pointed out by another referee that HALOE measurements in April do not extend south of ∼60S or north of ∼75N (Our original figure contained extrapolated results.) We therefore modified the figure to span only the latitude range of the HALOE measurement.

"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?"

The regressions used in this paper did not rely only on 5 points, but on 60. That is, we did not try to evaluate the relationship between model ozone and STE for each month individually. Our method actually appears to be the one suggested by the referee here. The extratropics is defined as the region where the mass of the lowermost stratosphere exceeds 30 g cm^{-2}. Thus, the region is not fixed, but is determined by dynamics.

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

The last sentence of Section 4.6 is: "Overall, however, the midlatitude agreement of the model with the observations is better than at tropical stations." This conclusion results from comparing Figure 15 with Figure 17, and we feel is correct. We have changed the revised version to explicitly refer to Figure 15, to avoid any confusion.

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

The stations included in Fig. 15 are indicated in the text, where we state that all stations within 10 degrees of the equator are included. In the revised version, we now also indicate this in the figure caption. We have also more explicitly specified which stations are included in Fig. 17, both in the text and the figure caption.

"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

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be useful."

Note on page 1603, line 4 in the ACPD version: "Figure 12 shows percent differences between the model and observed monthly mean profiles for the three stations shown in Fig. 11." We have fixed the caption in the revised paper. Variability is now shown on the figure.

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

Fixed in revised version.

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

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Interactive Comment