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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 the referee comments to which we have responded in italics, followed by our response in regular font.

Specific Comments:

"... tropopause ozone has got a substantial high bias, and the bias even increases when the ozone is evaluated relative to the position of the tropopause. I find this a somewhat surprising result, given that the RTT averaging technique is meant to make the analysis insensitive to errors in the tropopause height, which according to figure 4 should be small in many places, anyway."

The idea of RTT averaging is that there is a more-or-less canonical ozone profile with respect to the tropopause, which is simply displaced up or down by tropopause height variations. Averaging in a RTT coordinate system would remove the effects of tropopause height variations, and provide a clearer comparison between model and observations. However, there is no reason that there must be improved agreement between the model and the observations. What we find is that the RTT monthly averages retain the vertical ozone gradients seen in the individual sondes better than pressure averages. Model daily profiles do not have as strong vertical gradients, so when the RTT-averages are compared, the upper troposphere high biases increase. But we feel that the RTT-average does provide a more revealing comparison than the pressure average.

"I suspect that the results may depend a lot on the definition of the tropopause. The WMO definition, as used by the authors, is the obvious choice.... A sensitivity study, using a dynamical definition of the tropopause, would increase my confidence in the results."

We also think that it would be interesting to redo this evaluation using a PV- based definition of the tropopause. However, we believe that the errors involved would be excessively large due to the need to interpolate relatively low vertical and horizontal resolution analyzed meteorological data, and the noise due to the need to calculate derivatives of the velocity and potential temperature fields to obtain PV. We feel that our

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choice is justified by the results of Pan et al. (2004), which found that the tropopause layer centers on the thermal tropopause. This rationale is provided in the last paragraph of section 2.

"... some words about the implications of those large errors for coupled chemistry- climate modeling would be in order; after all, I think that quite a few models overestimate ozone in the NTR."

Ozone high biases near the tropopause, where temperatures are low, should result in an overestimate of the radiative impact of tropospheric ozone on surface temperatures. We have modified the discussion in the introduction to mention the potential overestimate in chemistry-climate models with ozone high biases near the tropopause.

"The conclusion that inadequate vertical resolution is to blame, remains a hypothesis until the authors actually perform a simulation with a model version with more levels in the NTR."

We agree that our focus on vertical resolution as the primary problem is a hypothesis. However, we arrive at this conclusion by evaluating and dismissing other possibilities such as excessive STE and too low horizontal resolution. In the revised version of the paper we also mention an experiment indicating that convection is also not a likely explanation. Repeating the simulations with higher vertical resolution would require generating a new meteorological data set with enhanced resolution, which is beyond the scope of this research effort.

"How does your vertical resolution compare to other models? ECHAM/MESSY has 90 levels, and you claim that your resolution is insufficient, so I wonder whether other models with better vertical resolution perhaps do better than yours."

Our vertical resolution near the tropopause is generally comparable to that of other models of this type. The ECHAM/MESSy model has a vertical resolution of about 600 meters in the UT/LS, which is nearly twice ours. The Mozart 3 model has vertical resolution in the UT/LS of ~1–1.2 km, depending on meteorological data used [Kinnison et al., 2007]. The TOMCAT/SLIMCAT models have about half the vertical resolution near the tropopause that we do. We have added a statement to this effect in the revised version.

"Section 4.2: A word repeating which tropopause definition was used, and why not others, would be in order here. At a second read it took me a while to find which tropopause you use."

We now specify in Section 4.2 that we are using the thermal tropopause in this analysis due to its high vertical resolution and availability of a temperature profile for each sonde with which the thermal tropopause can be identified. We also refer to the discussion of this issue contained in Section 2.

"Section 4.3, 3rd paragraph: Again, I think that at 4x5 degrees tropopause ozone may be better for the wrong reasons."

We do not mean to suggest that the 4x5 simulation is overall better than the 2x2.5 simulation. However, in the 4x5 simulation, high latitude tropopause ozone does agree better with the observations. We have revised the paper to make it clearer that there are compensating errors.

"I like figure 8. This is a sophisticated analysis which I haven't seen published before. 6th paragraph: I think your sign convention for the ozone flux is unusual. I would make it a positive flux of ozone into the troposphere of 266 Tg/year."

This is changed in the revised version.

"Section 4.4, last paragraph: Your conceptual model could be explained more clearly. It took me quite a while to understand what you were getting at."

We have revised the discussion in the revised paper to make it clearer.

"Page 1606, line 16 ff: Again, a counterintuitive result. I would have thought that at a

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higher resolution a larger amplitude would be likely."

It is true that the peak annual amplitude in the 2x2.5 simulation is larger in magnitude than the peak annual amplitude in the 4x5 simulation. However, the peak occurs at 15N - which is north of the tropical stations included in this analysis. The peak in the 4x5 simulation is latitudinally broader due to higher horizontal diffusivity and this results in the larger tropical amplitudes seen in the 4x5 simulation as compared to 2x2.5. We have expanded the discussion of this point in the revised paper.

"Also a supposedly better, stronger upwelling would form part of a stronger stratospheric overturning, which would then be associated with more STE and increased tropopause ozone."

Please note that we are discussing the annual amplitude of the upwelling. It is the annual average of the upwelling which would be associated with more STE and increased tropopause ozone. It is possible for the annual amplitude to be stronger without affecting the annual average STE.

"P 1609, line 19: "The model tends to underestimate the transition" This sentence does not make much sense to me. How about " The model's transition region is too deep" or "The model underestimates the curvature in the ozone profiles at the tropopause."

The wording changed following the suggestion of the referee.

"Is it possible to back up your theory about the role of vertical resolution with some sensitivity experiments?"

The Combo model adopts the vertical resolution of the meteorological data driving the model. There are currently no available higher-resolution meteorological data sets which have been converted to the necessary format. Work is under way to create a GEOS 5-based data set, which would have higher vertical resolution near the tropopause. However, this work will not be completed for some time. Sensitivity studies are thus not feasible at this time.

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"Figure 12: The caption does not mention Hohenpeissenberg (central column)."

This is fixed in the revised version.

"Figure 13: The caption does not explain what the blue and green bars stand for."

It is important to understand the difference between the green and blue bars. After RTT-averaging, one has the option of comparing the profiles at the same fraction of their respective tropopause pressures, or comparing in a pressure coordinate by normalizing each profile using the monthly mean or median tropopause pressure. If there are substantial differences in the modeled and measured tropopause heights, these choices can produce different results. If the model tropopause is above the observed tropopause, we found that comparing in a pressure coordinate produced closer agreement. The caption has been corrected in the revised version.

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

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