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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 #1

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Summary: The authors evaluate the Combo model's ability to reproduce the near-tropopause distribution of ozone, using ozone sonde data. The model produces some excellent stratospheric ozone, and tropopause heights are generally well captured. Nonetheless, 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. Also the result actually gets worse with improved resolution, which likewise is somewhat surprising and suggests that at low resolution there are compensating problems at play, so that transport errors and model diffusivity conspire to produce a better field than at higher resolution. Also 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, because it can be inferred solely from sonde data. However, adiabatic transport can on its own change the stratification, giving rise to an apparent change in tropopause height without any diabatic, chemical, or mixing process operating. Using a dynamical definition of the tropopause (based on PV) would be a well-tested alternative, although a derivation of PV along sonde tracks requires high-resolution meteorological data to be available. Ozone and PV are known to be highly correlated in the NTR, so one would then effectively study errors in the relationship between these two quantities. A sensitivity study, using a dynamical definition of the tropopause, would increase my confidence in the results. Given the importance of the tropopause region for the radiation budget of the atmosphere, 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. The authors study the effects of improving horizontal resolution, and find slightly puzzling results. 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. For my liking the paper is a bit too long for the amount of information contained.

Minor comments:

Section 3: 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.

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.

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Section 4.3, 3rd paragraph: Again, I think that at 4x5 degrees tropopause ozone may be better for the wrong reasons. I agree with you that perhaps in your driving wind fields the Brewer-Dobson Circulation may be too strong, which is counterbalanced by overly large diffusivity. This would explain that things get worse at the better resolution, but, as you showed, cannot on its own explain the high biases versus observations at both resolutions.

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.

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.

Page 1606, line 16 ff: Again, a counterintuitive result. I would have thought that at a higher resolution a larger amplitude would be likely. I don't understand how you arrive at the estimate that the upwelling should be 20% weaker than in observed. Your explanation appears somewhat vague and speculative. 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. So it might make things even worse at the tropopause. Also perhaps you can convince yourself that indeed your upwelling is insensitive to resolution, by analyzing your model results.

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

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

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

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

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