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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.

Anonymous Referee #2

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This paper presents an evaluation of the NASA Global Modeling Initiative (GMI) Combo model. The focus is the near tropopause ozone. For the main part of the paper, the model is evaluated using ozonesonde data from 23 stations. The authors presented a comparison of two averaging approaches that use different vertical coordinates. Given that there are previous publications of GMI combo model evaluation (e.g., Strahan et al., 2007), the unique strength of this paper is the focus of how well the model represents the ozone distribution near the tropopause and the idea of using the relative to tropopause coordinates in comparisons with observations. The paper is well structured and generally well written. In my view, the method of model evaluation discussed in the paper is equally important, if not more so, to the actual results of evaluation. The discussion on what we could learn from the different averaging approach is weak. Additional weakness is that the results presented seem to be inconclusive beyond the specific differences between the model and the data. Several specific weaknesses are described below. Suggestions are given for the authors to consider in their revision.

Weaknesses and suggested revision:

1. After reading, the general feeling is that there are many details from the comparisons and what exactly we have learned from these comparisons are not as clear. For example, the results show that there is a lack of ozone gradient cross the tropopause. The authors suggest that this is a sign of model being too diffusive and there is a need to increase the vertical resolution. On the other hand, when the 2.x2.5 run showed worsened high bias in modeled ozone, the authors suggest that this is due to not enough horizontal diffusion when denser grid points are used. I find these statements too scattered. It would work better to have a focused discussion on what we have learned about the effect of different grid resolutions to the near tropopause ozone. It is also important to state what the main goals are, beyond the details, when evaluating the near tropopause ozone in GMI model, and whether the goals are achieved. The abstract in particular could use some improvement to make the take home massage stand out better. For example, the authors proposed to use the RTT coordinates and have shown that the results of comparisons using the two coordinate systems are different. It would be useful to state in the abstract and the conclusion what we have learned using the RTT that we could not have learned using the regular pressure average.

2. In the discussion of tropopause ozone, some significant details are missing. Since the model has a coarser vertical resolution than the sondes, it is important to state in the paper how tropopause heights are derived in the model data. For example, is the derived tropopause level between model vertical grid levels or limited to the grid levels? If between the grid levels, are the ozone profiles from the model re-interpolated to this

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level or taken as a layer average? These details are important, especially because there appear to be a systematic high bias in the model tropopause ozone. Given the steep gradient of ozone across the tropopause, systematic bias can be induced by the bias in tropopause height determination. In addition, the variations of monthly tropopause pressure, as indicated by the vertical bars in figure 4, are unrealistically small. Take a mid latitude station, Wallops Island for example, the tropopause there often jump between the tropical and polar altitude depending on which side of the jet stream it is at, especially during Spring and Fall. It is hard to believe that the standard deviation of the mean is within 25 hPa. How much model data went in the tropopause height calculations? As many as the days with available ozonesonde data?

3. The issues discussed in the previous point suggest that in many ways the tropopause ozone may not be the best choice as an indicator of how the model is doing. Typically ozone has a steep gradient near and right above the tropopause, and which lead to large uncertainty in determining ozone value at the tropopause level. This uncertainty and associated variability is well illustrated in Figures 9 and 10. As shown by the black dots in Figure 9b, the ozone value at derived tropopause surface varys from 40 to 300 ppb. The average derived from this spread is not a very meaningful quantity. (I am not sure why 2 sigma showed up as ˜ 50 ppbv in figures 4 and 5. More on this in next paragraph). It is more meaningful to consider layer averaged quantities immediately above and below the tropopause.

4. The comparisons focused too much on the mean, not enough on the variability. Although the standard deviations are given in several figures, they appear to be unrealistically small or inconsistent. Take figure 4 as an example, the standard deviations for the model tropopause is given without mentioning how the statistical set is formed. The standard deviations for the corresponding observations are not shown. Another example is figure 11. Without error bars, it is not clear how the 3 sets of statistics relate to each other.

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