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***Interactive comment on “Comparison of CMAM simulations of carbon monoxide (CO), nitrous oxide (N<sub>2</sub>O), and methane (CH<sub>4</sub>) with observations from Odin/SMR, ACE-FTS, and Aura/MLS” by J. J. Jin et al.***

**J. J. Jin et al.**

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Reply to Referee 3

General: In my preliminary comments I discussed that this is an overly long paper and the figures are too hard to read. Specifically I criticized the use of so-called postage stamp plots like Figures 1-6 and 8. I said that The authors are attempting to present too much information and as a result making it incoherent. I personally think 20 sub-panels is excessive and I can not guarantee a thorough review if I have to wade through those. Regrettably none of these comments were acted upon and thus my initial assessment stands. Indeed, by adding another postage stamp figure (9 which was not in the initial

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manuscript) the authors worsened the problem! I suspect that scientifically, the paper is probably OK; although it is primarily a model documentation and validation paper. This is useful to do, but there is not much new that is introduced to justify all the material presented. The possible role of diffusion in the upper stratosphere might be one such topic, but it is only suggested and not demonstrated.

— We appreciate the comments and apologize for the delay of the modification.

— We agree with comment on the figures from both of the referees and cut the figures and panels in this modification. We removed the Figures 2, 5, and 8. As to the Figures 1, 4, and 7, we removed the panels for January and October and only show the distributions in April and July, so that the discussion is not sacrificed. We also reduced the number of panels in Figure 3 and 6 from twenty to five, so that the quantitative difference between model results and measurements is demonstrated. As to the Figure 9, only two panels are shown in the revised version. Moreover, we also removed the Figures 12 and 15, and cut the panels in the Figure 14. Correspondingly, the text has also been significantly modified.

Thus my first specific comment is 1) The statement in the abstract that quote: these negative biases can be reduced is not substantiated and thus should be changed to emphasize that it is merely a suggestion, not a demonstration of diffusion's importance.

— We moved this claim to the summary and emphasize it is merely a suggestion.

2) As far as suggestions for editing, I note that 3 figures and a total 56 panels are devoted to each of the CO and N<sub>2</sub>O comparisons; for CH<sub>4</sub> it is a bit less because MLS and ODIN do not measure CH<sub>4</sub>. But this is too much. Right away they should delete figures 2, 5, and 8. There is no information in them which is not in the other figure pair for each constituent. Then Figure 9 should be reduced to just a couple of panels since the conclusions follow straight from Figure 6.

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— We significantly reduced the panels in this figures, please see our reply above.

3) A second comment related to editing is that much of the difficulty in reading this paper relates to the fact that the authors show all three datasets. This introduces confusion as to whether the paper is a model-data comparison or a data-data inter-comparison. I suggest that the authors limit the plots of each dataset unless there is a significant disagreement between ACE-MLS-ODIN.

— We think it is an advantage that multiple datasets are used to evaluate the model so that reader can get a feeling of uncertainties of the observations from the plots for each datasets. However, we do limit the discussion on observation-observation inter-comparison to reduce the confusion.

4) Continuing on the theme of the need for editing, the authors devote 4 full paragraphs and over 20 references just to introduce Section 6! Surely the issue of polar descent can be described in half the verbage. They also should delete Figure 12. Its nice, but not relevant to the issue of how CMAM is doing.

— We shrunk the introduction part to two short paragraphs and removed the former Figure 12 and related discussion on this figure.

5) For Section 7, I had a hard time with Figure 14. What are "mean anomalies"? (same question applies to Figs 15 and 16). Are panels B and C subsets of A? What new information is given in panels B and C and why do the authors need to include them. The color scales are unreadable on an 8.5 x 11 inch paper. Again, this speaks to the issue of dumping data on paper without consideration for whether it has a purpose.

— We explain the caption more clearly: "Multi-year average of the tropical CO anomalies of MLS (panel A) and CMAM (panel B). The anomalies are the daily zonal means minus annual zonal means.", and we also only show two panels here in the revised paper (the former panels A, C, D, and F are deleted).

Finally, if the CO tape recorder is due to biomass burning, why show this at all since

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CMAM has no biomass burning source? If you want to validate tropical upwelling, why not show H<sub>2</sub>O?

— CO tape recorder is not merely due to the biomass burning (Schoeberl et al. 2008). That is also why we use it in this paper. As to the water vapour tape recorder, a separate study on it is being conducted.

I think this work eventually could be published, but it needs much work. If they tend to the above comments and subject the paper to a serious, critical editing, I would be pleased to review the revision. My comments above are by no means inclusive because I do not believe the text at present merits a complete, comprehensive review.

— We appreciate the referee's effort to improve this paper and we think a substantial editing has been conducted. Certainly we are looking forward to further comments on this revised version.

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Interactive comment on Atmos. Chem. Phys. Discuss., 8, 13063, 2008.

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