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Comment

Interactive comment on “Comparison of CMAM simulations of carbon monoxide (CO), nitrous oxide (N₂O), and methane (CH₄) with observations from Odin/SMR, ACE-FTS, and Aura/MLS” by J. J. Jin et al.

Anonymous Referee #3

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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

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stands. Indeed, by adding another postage stamp figure (#9 which was not in the initial 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. 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.

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 N₂O comparisons; for CH₄ it is a bit less because MLS and ODIN do not measure CH₄. 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.

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.

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.

5) For Section 7, I had a hard time with Figure 14. What are "mean anomalies"? (same

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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. Finally, if the CO tape recorder is due to biomass burning, why show this at all since CMAM has no biomass burning source? If you want to validate tropical upwelling, why not show H₂O?

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.

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

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