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Interactive comment on “Modeling the effect of plume-rise on the transport of carbon monoxide over Africa and its exports with NCAR CAM” by H. Guan et al.

Anonymous Referee #2

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General Comments:

This paper presents results of model simulation of CO during 2 weeks of September 2000 both with and without a plume rise model for fire emissions embedded in the NCAR Community Atmosphere Model. The plume rise model is that used by Freitas et al. [2006, 2007] in the Brazilian version of the RAMS regional model. Guan et al. compare results with the mean of aircraft data for one day over Africa and two days over Australia and with MOPITT data (3 day mean). This study is very similar to the previous work of Freitas et al., and does not really offer any more extensive evaluation of the plume rise model (less in fact). It is clearly of interest to better simulate the

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emissions and transport of CO from fires using an interactive approach, and the work of Freitas and colleagues has been a major step forward in this regard. However, the present study does not add much insight over and above the two papers by Freitas et al. [2006, 2007]. The only difference is that the present paper uses a global model and examines a different period, but it focuses on the same geographic region as the work from which it is derived. I would have been much more interested to see a larger scale study of issues in the literature such as the typical underestimate of MOPITT CO in the southern biomass burning season, or difficulties matching the seasonal variation, or in matching both the tropical data and the southern extratropical data. Are these issues affected by the use of the plume rise model?

I felt I was left with more questions than answers by this paper, and I gained very little insight from it. If there was extensive sampling of smoke plumes in SAFARI 2000 (Section 3.3) why are comparisons made for only 1 day over Africa? There were aircraft data from TRACE-A in 1992 near African savanna fires that showed vastly more CO in the boundary layer than above it. Why are the data in Figure 6 so different? Why was the model run for such a short period, particularly one so late in the burning season?

There are a number of instances of improper English usage in this paper. It should be edited carefully by one of the native English speaking authors.

Detailed comments:

p. 18146, l. 10. The abstract states that the model with plume rise shows substantial improvement of the agreement of the model with aircraft profile data, but this rests on comparisons for one day.

p. 18147, l. 10 on. This paragraph summarizes some previous observations and model studies. However it is written as though the work showing that fire emissions reached the stratosphere were simply a result of the energy from the fuel consumed in the fire. Careful reading of the papers, particularly the observations and analysis of Fromm and colleagues, will show that the particular meteorological situation was also

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an important contribution to the emissions reaching the stratosphere, sometimes with nearby convection playing an important role. Also, the text lumps together studies of boreal fires with the work by Freitas et al. on tropical fires, many of which have very low energy input. It is really only the deforestation fires in the tropics that can have energy input similar to those in stand-replacement fires in the extra-tropics.

p. 18147, l. 15. Text implies that biomass burning takes place in the free troposphere!

p. 18147, l. 26. Cite Freitas et al., 2007 after size of fire, as they investigated all the factors listed.

In the introduction, explain why such a short period, only 13 days, was examined.

P. 18149, l. 9-16. Explain why a potentially inconsistent set of OH fields (from the GISS model) and CO emissions from an inverse study with MOPITT were used. Obviously the agreement (or lack thereof) with MOPITT data depends on the sources and sinks of CO.

P. 18149, l. 18-19. You really don't need to cite all these papers here, it is really well known that there are large biomass burning sources in the tropics. If you have to cite something, stick to the pivotal papers like Crutzen, 1979, and a recent paper that is actually on emissions estimates like the GFED2 inventory paper (van der Werf) if you need a recent one.

P. 18149, l. 29. Why use daily MODIS? There are huge gaps. Why not some kind of running mean over several days? It could really corrupt results for the very limited case study shown here.

P. 18150, 16-25. Why all this discussion as if you made a decision to limit the analysis to 3 vegetation classes? It is simpler to say that you just adopted the approach of Freitas. Also, Freitas et al. [2007] showed the effect of fire size, heat input etc., and say that you used the IGBP land cover, and give the grouping. If the IGBP "savanna" class is the savanna without trees or shrubs, it should have been put in the grassland category,

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which will affect the adopted heat flux which depends on fuel load. Say which values you adopted for heat input, rather than just quoting the range in the Freitas papers.

P. 18151, l. 15, 24. The MOPITT papers are by Deeter, not Deeler and Deer.

P. 18151, l. 20. You must mean "transformed" not "transferred".

P. 18151-52, l. 24 on. L2 retrieval surely, L3 usually refers to e.g., monthly means of the retrieved quantities. L2 refers to individual retrievals. Was the model output treated properly, i.e., the model sampled at the location of each MOPITT profile, and then the averaging kernels (AKs) and a priori applied to individual model profiles, and then the results averaged over the 3 days and a $1^\circ \times 1^\circ$ in Figure 2? The text needs clarified, as it refers to the MOPITT "grid" being $1^\circ \times 1^\circ$. Usually when AKs are applied, they are applied to individual profiles in a model (even though that model profile is on a coarser grid).

P. 18152, l. 10. Drop the Richards et al. reference, it has not been published in ACP even though the reviews in ACPD were submitted over a year ago. There are better papers by in JGR showing MOPITT-model comparisons, e.g., by Arellano.

P. 18152, l. 12. It is not at all clear from Figure 2 that agreement with MOPITT data is better when the plume rise model is used, change the text accordingly. Indeed, at the bottom of the page, the text states that the plume rise model does not substantially improve the model-MOPITT difference, a more realistic appraisal.

P. 18152, l. 23. The text doesn't make any sense, rewrite.

P. 18153, l. 4-15. The authors seem to be trying to lay the blame for the model underestimate of MOPITT data at the quality of the MOPITT retrieval. This is a bit odd, as there are now papers showing that other models underestimate CO in the southern biomass burning season (e.g., Arellano et al. papers), and that when TES and MOPITT data are treated consistently, they agree. Given the uncertainty in biomass burning emissions, and in the treatment of convection in the models, etc, etc, we really don't know

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why the models have this problem, so please clarify, instead of trying to blame it on model resolution, gaps in the MODIS data, or the quality of the MOPITT retrievals.

P. 18153, l. 16-18. Cape Point is not a CMDL station. (CMDL isn't even CMDL anymore, it is NOAA/GMD, but that is not the issue here.)

P. 18153, l. 20 on. To use 12 days of data at Cape Point to say that the model simulates background CO well is a bit of a stretch. It is better than nothing, but that is about all.

P. 18154, l. 2 on. Give a reference for the CO data. Discuss why only one day of data was used. How about the CO data shown in McMillan et al., 2003 (SAFARI special issue of JGR) for September 7? Are the CO data in Figure 6 shown in any more detail in any publication? If the model resolution is $2^{\circ} \times 2.5^{\circ}$, why average the data over $4^{\circ} \times 6^{\circ}$ in Figure 6? At least show results for each model grid. The same comment applies to Figure 7. What is the spatial scale of Figure 5? The region with the fires in Figure 5 is a region of grassland, so why is there enough energy to cause substantial plume rise? From Figure 4 in Freitas et al. [2007] the plume rise would be less than 3 km.

I am very surprised that Figure 6 does not show higher values in the boundary layer if these data are in the vicinity of fires. Were these data taken directly over fires?

P. 18156 on. I found the discussion of export to the Atlantic to be very qualitative. As in line 15, the area with $\text{CO} > 300$ ppb is "much larger" with plume rise. It isn't really very much larger in Figure 8.

Figure 9 shows that the fire emissions were much greater over S. America than over southern Africa. So why is CO so much higher over Africa?

I have no intuitive feel for fluxes in kg/sec, so it is hard to tell what the significance of the differences in flux with and without plume rise are. I suggest commenting on the fractional difference in export fluxes through a plane. Certainly Figure 11 shows very little difference in mixing ratios over most of the Indian Ocean.

References.

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