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Interactive Comment

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.

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We appreciate the valuble comments and are grateful for your time and effort. The followings are our responses to the comments.

General comments:

We think the topics you listed are very interesting. Currently, we are studying the effect of plume-rise on different geographic regions, which is mentioned in the future work of the original manuscript. One of our major purposes in this paper is to conform if the effect of plume-rise effect in a global model is similar to the the effects of plume-rise in a regional model. So we focus on the similar region. To offer more information we have added Section 4.3 in the paper to address the effects of plume-rise on CO budget over southern Africa. The paper is chceked by the native English speaking authors.

Rest of the general comments are similar to the Q1, Q5, and Q20 in the detailed comments.

Detailed comments:

Q1) 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.

A1) Simulation of biomass plumes with high temporal and spatial variations is a difficult task for a coarse-grid global model. Relatively low spatial coverage of flight tracks adds an extra challenge for our model-aircraft comparison. In order to illustrate the effect of plume-rise parameterization on vertical distribution of CO, we select September 3, 2000 as our comparison case, which is a relatively-well simulation case during our simulation period. Now "seems to show" is added in the text."....plume rise seems to show substantial improvement...."

Q2) P.18147, l. 10. 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 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 extratropics.

A2) We agree that the meteorological situation combined with substantial energy re-

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lease could loft the fire emissions to the stratosphere. However, our focus is not on what or how other mechanisms loft the fire emissions to the stratosphere. This paragraph is to illustrate that the use of a constant injection height in global model is not realistic. We show the real injection height is highly variable with regions by listing the range of injection height from tropical fires (low energy) to boreal fires (high energy and high instability).

Q3) P.18147, l. 15. Text implies that biomass burning takes place in the free troposphere!

A3) "in the free troposphere" was removed.

Q4) P.18147, l. 26. Cite Freitas et al., 2007 after size of fire, as they investigated all the factors listed.

A4) Done

Q5) In the introduction, explain why such a short period, only 13 days, was examined.

A5) The following statements are included in the introduction. "In this study, we illustrate the regional effects of plumerise parameterization on the vertical distribution of CO and its exports to the Atlantic Ocean and Indian Ocean by a global model. The "Great African Plume" (Chatfield et al., 1998) and "River of Smoke" (Sinha et al., 2004) are two important plume events or export pathways during the dry season. Our 13-day simulations capture the two major events observed during the SAFARI 2000 Dry Season Campaign of September 2000 (Swap et al., 2003). These events involved relatively short transports from the sources in southern Africa and so 13-day simulations with one-year spin-up appeals sufficient for the details we needed. We focused more on mechanism than budget analysis.

Q6) 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

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and sinks of CO.

A6) We agree that using both of OH concentration and CO emissions form an inverse study would give a more accurate comparison with MOPITT data. However, the author (Pètron) was not able to find the OH data necessary to perform such a simulation. Since CO has a long lifetime and 3-D source and sink are also significantly smaller than the surface emission (Section 4.3), even large difference (25%) in OH will have small effects in these primarily mechanism story.

Q7) 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.

A7) Following your suggestion, now we only cite the two papers you mentioned.

Q8) 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.

A8) Fire activity displays highly temporal variation. Daily ABBA fire count data shows that the day-by-day variation over Central American fire region can reach to 100%. So using running mean over several days could significantly underestimate the peak emission, therefore, CO concentration. On the other hand, MODIS gap is small compared to the large fire regions during the simulation period. We feel it is more important to simulate high-intensity events with approximately correct frequency than to simulate a low-intensity mean.

Q9) 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, 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.

A9) The text was simplified as: "We adopted the approach of Freitas et al. (2007) to aggregate the classes into three major vegetation types: forest, savanna and grassland." The IGBP savanna class is the savanna with trees or shrubs. Both of minimum and maximum heat fluxes are used to calculate the plume base and top in the1-D plume model. So we give the used minimum and maximum heat fluxes in the text.

Q10) P. 18151, l. 15, 24. The MOPITT papers are by Deeter, not Deeler and Deer.

A10) Done

Q11) P. 18151, l. 20. You must mean "transformed" not "transferred".

A11) Yes, you are right.

Q12) 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°x1° in Figure 2? The text needs clarified, as it refers to the MOPITT "grid" being 1°x1°. Usually when AKs are applied, they are applied to individual profiles in a model (even though that model profile is on a coarser grid).

A12) MOPITT L3 files also contain daily mean gridded (1°x1°) versions of the daily L2 CO profiles. The transformed model output was compared with this data-set. The model output was transformed with L2 AKs and a priori. We have included the following suggested sentences in the text: "The model outputs were sampled at the location

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of each MOPITT profiles, and then the L2 averaging kernels and a priori applied to individual model profiles, and finally, the results were averaged over the 3 days and each 1°x1° grid."

Q13) 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.

A13) The Richards et al. reference was dropped and the paper by Arellano was cited in the text.

Q14) 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.

A14) Now "slightly better" is used. "The plumerise model does not substantially improve the model- MOPITT difference was replaced by "The plumerise model does not complete resolve the model-MOPITT difference."

Q15) P. 18152, l. 23. The text doesn't make any sense, rewrite.

A15) The text was rewritten as "implying that simulation of CO mixing ratio within or near source regions is particularly difficult."

Q16) 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 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.

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A16) The statements associated with the quality of the MOPITT retrievals were removed. We also added the missing NMHC contribution and inconsistent OH and CO emission data as extra error sources.

Q17) 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.)

A17) CMDL was removed.

Q18) 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.

A18) Note that the time variation of background CO is very small. We checked the ground-based CO concentration at Cape Point over a longer time period (from 15 August to October 15, 2000) and found that CO concentration at Cape Point only displays small variation (58 - 63 ppb) during two month period. The CAM simulation also shows the CO plume does not pass through this site during simulation period (not shown), which should represent typical background value.

Q19) 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°x2.5°, why average the data over 4°x6° 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.

A19) The reference by Piketh et al. (2004) was cited. We have not found any publication which shows more detail about the CO data in Figure 6. The statements of A1 are

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included in the text in order to address why only one day of data was used. The flight distances are longer than model grid distance. So we used the averaged model data over several grid points. Now the model spreads are also included in Figure 6 and 7. The spatial scale of Figure 5 is about 4°x 4°. The region with the fires in Figure 5 is a region of savanna land, which has a higher heat flux (23 W/m2) than the grassland (3.3 W/m2) in the 1-D plume model. Therefore, the plume could reach a higher altitude. On the other hand, the previous studies (Magi et al., 2003; Schmid et al., 2003) also show that the "river of smoke" event resulted in a heavy pollutant layer from the surface to 5 km.

Q20) 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?

A20) Fire size distribution as estimated by the WF_ABBA algorithm (See Fig.1 in Freitas et al. 2006) shows 75% of the detected fires have a fire size less than 20 ha (0.2 km2). The horizontal distance of the flight is ˜ 500 km. Therefore, these data were not always taken directly over fires.

Q21) P. 18156 on. I found the discussion of export to the Atlantic to be very qualitative. As in line 15, the area with CO $>$300 ppb is "much larger" with plume rise. It isn't really very much larger in Figure 8.

A21) To make the discussion more quantitative, the following sentence was included in the text. "The ratio of the area where the CO mixing ratio exceeds 300 ppb between two runs is 1.81."

Q22) 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?

A22) The figure shows the CO emission rate at 00 UTC, which corresponding to different local time in South America (late afternoon) and the southern Africa (night). The emission rate is dependent on local time.

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Q23) 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.

A23) We have calculated the total CO fluxes through 4 planes although other publication also prefers "kg/sec" (Sinha et al., 2004). We have added CO fluxes in a new Table 1. The results are also discussed in new Section 4.3.

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