Atmos. Chem. Phys. Discuss., 7, S9857–S9864, 2008
www.atmos-chem-phys-discuss.net/7/S9857/2008/

Atmospheric Chemistry and Physics Discussions

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.

H. Guan et al.

Received and published: 10 July 2008

We thank to the reviewer for her/his comments and suggestions. Many are very useful in improving the manuscript. We have modified our manuscript in response to these comments and suggestions.

Major Comments:

A very useful addition to the paper would be a budget analysis of CO over Africa. In such an analysis all the budget terms are analyzed for the two simulations. These terms include the initial burden, fnnal burden, emission, transport to the east, west, south and north, and transport fluxes through certain vertical layers (e.g. the transition between boundary layer and free troposphere). Finally, the 3D production (by NMHCs?) and

oxidation by OH should complete the budget.

A) We have conducted budget calculation of CO over southern Africa. The result is described in Section 4.3. Instead of the vertical flux through the certain vertical layer, we have calculated the CO burden below and above the certain layer. The plumerise parameterization instantly distributes the biomass emission into the layers between the plume bottom and top. Therefore, the vertical flux is not able to reflect this process.

Specific Comments:

Q1) Title: I do not see how Transport over Africa is studied in the paper. The effect of plume rise on CO export is studied. So I suggest: Modeling the effect on plumerise on the Carbon Monoxide export from Southern Africa.

A1) The effect of plume rise on vertical transport of CO is studied in this paper. So we feel the original title is appropriate.

Q2) Abstract "The scheme was first adapted from a regional model." Unclear and not relevant for an abstract.

A2) Thanks for the suggestion. The sentence was removed.

Q3) Introduction P18147: l1: a sensitive tracer of incomplete combustion: a tracer indicative for incomplete combustion

A3) Done

Q4) L28,29 : to create the correct injection height for biomass burning : to calculate/estimate

A4) Changed to "to calculate".

Q5) P18149, l10: Non methane hydrocarbons (NMHCs) are not included as a volume source of CO? This prevents a proper comparison with available CO observations, since oxidation of NMHCs constitutes an important source for CO, especially in the

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tropics.

A5) This simulation did not include volatile organic carbon (VOC) emissions as precursors for CO in order to focus our discussion. Two types of VOC precursors are important for detailed global budgets. VOC components accompanying CO emissions in biomass burning plumes are quantitatively minor with respect to CO itself (Andreae and Merlet, 2001), and typically produce most CO within 1-2 days. Quantitative uncertainties in the CO emission source strength itself are of more consequence. VOC emissions from tropical regions are dominated by isoprene and some terpene emissions from trees. This publication is illustrative and did not include them either. Our study concentrates on the burning season, a dry season in the Southern Hemisphere when emissions are minimal. Our experience is that the effect of natural VOC emissions tends to create broad increases in CO over and near to actively photosynthesizing forest regions. Of course, the effect of adding any VOC emissions would increase the flux from southern Africa slightly. We included the following statements in the text. "Similar to the recent work by Arellano et al. (2007), we neglect the CO source due to oxidation of NMHCs, which is expected to have a small effect for this major focus on transport mechanism study."

Q6) L27:"daily fire count data as a solution". The authors should indicate some major limitations of this approach also. (i) the 10:30 a.m. and 10.30 p.m. overpasses tend to miss the most active fires in the afternoon (ii) simply using fire counts to distribute the monthly emissions (what are they?) is prone to errors due to e.g. cloud contamination of the MODIS observations.

A6) The limitations of this approach were mentioned in the last paragraph of section 3.1. P16153: l8 and 19: some temporal fire events was changed to the most active fires in the afternoon. Monthly emission is the monthly biomass burning emission derived by Pétron et al. (2004)

Q7) P18150: l28: "Preliminary test runs, using fire sizes of 10, 20, and 40 ha have

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shown that the patterns and magnitudes of simulated biomass plumes"; Since the parameterisation is essentially 1D, I do not see how the horizontal are size is taken into account in the subgrid parameterisation. Some more explanation is needed if you mention the fire size.

A7) The following statements were included in the text: "Fire size affects the plume-rise height through decreasing the role of entrainment. In the 1-D plume model, the entrainment coefficient is reversely proportional to fire size."

Q8) P18151:"at local afternoon (13:45 p.m.)". I think it should be either 1.45 p.m, or 13.45.

A8) Done. "1:45 p.m."

Q9) P18152: "Both of the simulations and MOPITT measurements also show that CO flowed into the Southern Atlantic Ocean from southern Africa directly from the east."; Strictly spoken, this is not correct. Figure 2 only shows that high CO concentrations are present, but it does not show the flow. Caption Figure 2;White areas are regions not seen by MOPITT during this period";: White areas indicate regions where the MOPITT observations are obscured by clouds.

A9) We agree. The statement was changed to: "show the high CO values over the Southern Atlantic Ocean". "White areas are regions not seen by MOPITT during this period" was replaced by the suggested sentence.

Q10) P18152, l28, "As the results indicate, the plumerise parameterization does not substantially improve the difference between the retrieved model and MOPITT CO at 700 hPa level." This statement is a bit unclear in the sense that first the focus is on improvements in the fit, and now the results are marginalized. I would replace "not substantially improve"; by "not complete resolve". The main point, that including plume rise works in the right direction, should more clearly be made.

A10) "not substantially improve" was replaced by "not complete resolve"

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Q11) P18153: l1 "the retrieved CO concentrations also include the contributions from other levels, the increased CO concentration due to the plumerise parameterization at 700 hPa level is partly offset by the decreased CO at lower levels"; The MOPITT averaging kernels decrease rapidly towards the surface. Without further investigation (i.e. consider modeled profiles, different MOPITT levels, etc.) this statement misses scientific backup and should be removed. One reason that could be added is the missing NMHC contribution.

A11) The associated statements were removed. The missing NMHC contribution was included in the following paragraph, which is considered as one of the potential reasons for the discrepancy between the modeled and MOPITT CO.

Q12) L12: "The approaches in selecting a priori profiles": Earlier it was mentioned that a single a prior profile was used in the retrieval.

A12) The statement was removed. See A11.

Q13) L15: "Consequently, better consideration of the profile retrieval process and utilization of additional data describing the retrievals would improve the accuracy in CO retrievals."; This statement is not substantiated. Is this a statement made in the Luo et al, 2007 paper, or by the authors?

A13) The statement was removed and the corresponding referees were also removed. See A11.

Q14) L24: "The good agreement between the model and ground measurement at Cape Point implies the simple formulations of the model can simulate the background CO" (i) Why are daily averaged data compared, and not hourly data? Now only 11 data points are left. (ii) The background CO is overestimated, while the important NMHC source is missing. Some explanation is needed here.

A14) (i) The reviewer feel that the hourly data would be significantly informative. However, only daily and monthly CO data are available at the WDCGG

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(http://gaw.kishou.go.jp/wdcgg.html). Furthermore, hourly observations at Cape Point are more likely to describe the extremely specialized meteorology of transport to this unique peninsula station than they are to describe features that can be resolved in this model.

Q15) P18154: L16: "Specifically, we would look at whether, within the vicinity of the flights, the model is capable of predicting plumes with similar mixing ratios and distributions." It remains unclear whether the model is sampled exactly at the times the aircraft took air samples. Since the campaign was specifically focusing on pollution events, not doing so could result in a significant bias. If co-sampling is not performed, it should clearly be stated.

A15) We have used the same time window for our model-aircraft comparison. We included "during the aircraft sampling period" in the following statement: "We focus on the comparison over the flight domains during the aircraft sampling period."

Q16) L23 "The CO vertical profiles around Sun Pan, Botswana (20-24ž S and 24-30ž E) for 3 September 2000 between the model runs and aircraft observations is compared in Fig. 6."are compared". It would also be instructive to include the model spread in fgure 6.

A16) Done. The model spreads were also included in figure 6 and figure 7.

Q17) P18155, l14: long-distance: long-range

A17) Done

Q18) P18156: l8: to reiterate: the transport is not shown in figure 2.

A18) The sentence was reiterated. "westward to the Atlantic Ocean as seen in Fig.2" was replaced by "westward in a latitude band between 15°S and 0° to the Atlantic Ocean (see Fig. 8)."

Q19) P18157: l11: "The CO depletion by plumerise process in the lower troposphere

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leads to less CO lofting by deep convection process". By the plumerise process.... by deep convection. This statement is not trivial and I would spend a few more words. Where does the deep convection take place? Probably in a different latitude (ITCZ) than the fires occur. That means that large scale transport should bring (i) the CO-depleted BL air to the updraft (ii) the air that has experienced deep-convection to the upper troposphere at 12S. Why does this phenomenon show a stronger feature over South America?

A19) The difference is mainly due to different meteorological condition between South America and the southern Africa. During September, the convection is prevalent over South America. However, the anticyclonic circulation pattern dominated over the southern Africa, which leads less convection activities.

Q20) L20: "The larger westward flux of CO for the PR run induced the higher middle-tropospheric CO in the Southern Atlantic Ocean (Fig. 9c)". This is not obvious: why could outflow from South America not play a role? From figure 9c itself that is not clear.

A20) The sentence was reiterated as "The larger westward flux of CO for the PR run lead to extra CO exported to the higher middle troposphere in the Southern Atlantic Ocean. "

Q21) L27: which dose: which does

A21) Done

Q22) L28: on the horizontal transport pattern: To be strict: on the horizontal transport pattern of CO. The general transport pattern is not influenced in an offline setup.

A22) Done.

Q23) P18159, l1: "by clean background atmosphere": by clean background air. Or: by the clean background atmosphere.

A23) Done. Now it is "by clean background air"

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Q24) L10: "Future work will simulate and understand" I would say: "Future work will focus". Promising understanding in this stage is a bit too much!

A24) Done.

References.

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