

Interactive
Comment

Interactive comment on “The Coupled Aerosol and Tracer Transport model to the Brazilian developments on the Regional Atmospheric Modeling System (CATT-BRAMS) – Part 2: Model sensitivity to the biomass burning inventories” by K. M. Longo et al.

Anonymous Referee #1

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This manuscript presents an interesting and important study on the effect of using different biomass burning emission inventories in a regional transport model to improve the tropospheric simulation of carbon monoxide (CO) concentrations in South America. Comparing model simulation with in-situ and aircraft measurements, the study indicates the importance of accurate information within a model regarding timing and location of fires in the emissions inventories in order to simulate realistically the time

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variability of near-surface associated air pollution.

Although the presented material is interesting and valuable, the approach adopted in the paper is not satisfactory. The reviewer recommends publishing the paper in ACP after some major corrections as suggested following.

General Remarks

1. My major concern is the methodology that the manuscript uses. The authors address CO simulations for the 2002 dry season using their real-time biomass burning inventories (3BEM) and two climatology inventories. Why do the authors only choose climatology inventories for their comparison for a specific study period? There are some referred year-specific biomass burning inventories available in our scientific community, such as Global Global Fire Emissions Database version 2 (GFED2) van der Werf et al., 2005] and GWEM-1.3 inventory (GWEM) [Hoelzemann et al., 2004].

2. The authors still need to clarify the CO module used in the simulation. The current study only considers two CO emissions from biomass burning and biofuel. Why does the study ignore the other emissions from fossil fuel and biogenic? How about CO produced by methane oxidation? If these processes are not important in comparison to biomass burning emission, the authors should give a general estimation of the simulated CO uncertainty due to missing these sources to justify the used approach.

Specific comments

1. Page 8572 line 23: What is the fraction of biomass burning emission of aerosol particles in South America to global total? This answer tells readers how important South America biomass burning emission is on the perspective of the global scale.

2. Page 8574 line 11-16: The road map is messed up. It does not correspond to the following text.

3. Page 8574 line 18-19: What are these new improvements for biomass burning emission parameterization? Why do the authors introduce these improvements? What are

the improvements of simulated CO concentrations due to using the upgraded emission parameterization?

4. Page 8576 line 11-13: Please give a rough estimation of the potential influence of ignoring emissions other than biomass burning and biofuel.
5. Page 8576 line 14: Add “climatology” before “biomass burning emission”.
6. Page 8577 line 21-23: The simulation started on July 15, 2002 and the results used for analysis started on August 1, 2002. The fifteen days spin-up time is too short for CO simulation since CO lifetime in South America could be a couple of months.
7. Page 8578 line 5-6: How do the authors treat CO dry deposition? CO dry deposition is small and was ignored by many previous studies.
8. Section 3: I suggest splitting subsection 3.1 into two subsections; one for model comparison with in-site surface measurement and one for comparison with aircraft measurement. This way, the manuscript has a structure for section 3 which is consistent with the task summary outlined in the prelude of section 3.
9. Page 8580 line 18-19: Figure 5 shows that model CO using biomass burning emissions of EDGAR and D2003 is much lower than the model CO simulated with 3BEM emission; however, this is contrary to what I observe from Figure 3. Figure 3 indicates that EDGAR emission is consistently higher than that of 3BEM. Adding the exact location and time of each flight presented in the Figure 5 derived from the experiment SMOCC/RaCCI would help readers understand the results better.
10. Page 8581 line 11-12: No. MOPITT retrievals are reported at 7 pressure levels, not 3. The authors only use 3 layers of MOPITT data.
11. Page 8581 line 19: Add a space between “D2003emissions”.
12. Page 8581-8582: Two sentences are repeated; one is on line 17-19, page 8581 and the other is on line 25, page 8581 to line 2, page 8582.

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13. Page 8582 line 3-4: Figure 7 and 8 also indicate that CO simulated by burning emissions from EDGAR and D2003 is lower than that of 3BEM. This is contradictory with the emission information revealed in Figure 3.

14. Page 8583 line 6-9: Rephrase the sentence.

15. Figure 4: For the scatter plot, where is the CODPSH data? Should the black dots be the red dots? Also the black number of “R2=0,66” may be “R2=0.66” in red.

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