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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.

K. M. Longo et al.

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Questions and answers to reviewer 2:

The manuscript by Longo et al., is the second of a sequence of manuscript introducing the CATT-BRAMS model. Part 1 (Freitas et al., 2007) deals with the model description and evaluation, while part 2 (Longo et al., 2007) investigates the sensitivity of the model simulations to biomass burning inventories. The present manuscript describes a newly developed system for the calculation of emissions from biomass burning, fo-

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Discussion Paper

cusing on fires in South America (3BEM). These fire emissions are incorporated into a three-dimensional atmospheric transport model (CATT-BRAMS) and the model results are compared to ground-based and airborne in situ and satellite remote sensing measurements of CO. In addition, two other emission inventories were included into CATT-BRAMS and their results are compared to those obtained using 3BEM and to CO measurements. Tropical biomass burning is one of the main contributors to the atmospheric composition during the dry season in South America, and an accurate description of biomass burning emissions is mandatory for atmospheric chemistry models. Therefore the scope of the manuscript (i.e., the presentation and evaluation of a newly developed biomass burning inventory) is well suited for publication in ACP.

In its present form, however, the manuscript does not contain enough original scientific material to merit publication in ACP without major revisions. Especially the separation between the two parts describing CATT-BRAMS is unclear. The main presentation of the modeling system and the evaluation of the model performance with respect to the meteorological parameters (e.g. temperature, humidity, wind, precipitation) and atmospheric tracers (e.g., CO and aerosol) is done in Part 1. Part 2 is supposed to deal with the ’Model sensitivity to the biomass burning emission inventories’. However, in wide parts the paper also deals with the evaluation of the model simulation (e.g., the abstract ends with ’…pointing out the reliability of the model from local to regional scales.’). Since no additional observations are considered for model evaluation in Part 2 compared to Part 1, no original information concerning the model evaluation is presented in Part 2. By including additional observations (e.g., from AIRS, MODIS and AERONET) for model evaluation the present manuscript could gain scientific substance. At present, the main scientific value of the manuscript lies in the presentation of 3BEM and the intercomparison between the different biomass burning emission inventories. However, this intercomparison is rather limited and should be extended substantially. I suggest to include the comparison with the GFEDv2 emission inventory already in the present manuscript (and not in an upcoming paper as suggested by the authors in the conclusions), and to deeper investigate the impact of the high temporal resolution of 3BEM, since this seem to be a rather unique feature of 3BEM. Enclosed are my specific comments that should be considered before publication of the manuscript in ACP.

A) We thank the reviewer for his/her insightful and helpful comments. The paper is now much improved by his/her comments and corrections. His/her contribution also provides us a better understanding of limitations as well as the robustness of our modeling system. We now include intercomparison with GFEDv2 and we also explore the impact of the high temporal resolution of 3BEM by including a monthly-averaged version of 3BEM.

Specifc Comments: - page 8572, line 17: Please replace ’pointing out’ by ’suggesting’;: From the very limited evaluation presented in the manuscript it is not possible to conclude that the model is reliable from local to regional scale. For this conclusion, a more comprehensive model evaluation and verification needs to be conducted.

(A) Done.

- page 8573, line 19 ff: Several new emission inventories for biomass burning have been developed and applied in recent years. More previous work should be mentioned here, besides Hao and Liu, 1994, GFEDv2, and Duncan et al., 2003. This could include Schultz, ACP, 2003; Generoso et al., ACP, 2003; Heald et al., JGR, 2003; Hoelzemann et al., JGR, 2004; Ito and Penner, JGR, 2004; Wang et al., JGR, 2006; Hyers et al., JGR, 2007.

(A) Done

- page 8574, line 19: It is stated that the new emission parameterization is based on a previously published method (Freitas et al., 2005, BFEMO) ’with several improvements’;. Since a significant part of the present manuscript deals with the description of the new method, 3BEM, the differences between 3BEM and BFEMO and if possible

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their impact on the biomass burning emissions should be clearly stated. While I assume that 3BEM includes an improved description of the biomass burning emissions, it would be valuable to see the impact on the model performance for CO mixing ratios.

(A) Performing simulations with the Freitas et al. (2005) parameterization was not possible and we don't think it is worth to include it in this paper. The expression 'with several improvements' was removed.

- page 8575, line 11ff: I assume that the land use and vegetation datasets provide only the carbon density and not the emission and combustion factors. Please modify this sentence accordingly.

(A) The sentence now reads:

The fire detection maps are merged with 1 km resolution land use data (Belward, 1996, Sestini et al., 2003) to provide the associated emission (EF) and combustion factors through a look-up-table. The corresponding carbon density is defined from the carbon in live vegetation data, estimated using (Olson et al., 2000 and Houghton et al., 2001).

- page 8575, line 11ff: Presumably the land use data set relies on a constant land use and does not include the ongoing transformation from rain forest to pasture. What is the reference year for the land use data set, and has the land use changed significantly since then? Please add a statement on the potential impact of land use changes on the calculation of biomass burning emissions.

(A) The updated data set from PROVEG is based on year 2000; this information is now in the paper. The following sentence was also included: 'One shortcoming of 3BEM methodology is the use of static land cover and carbon content data sets. Therefore, there may be a mismatch between the actual values and those prescribed by the data sets on pixels where fires took place previously or where the land use changed'.

- page 8575, line 14: Add the corresponding symbols from equation (1) after carbon density, emission factor, combustion factor to increase readability and attribution of the

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individual terms.

(A) Done.

- page 8578, line 14 ff: It seems that all emission and combustion factors are taken from Ward et al., 1992, so the reference to Andreae and Merlet, 2001, should be removed here.

(A) We actually used data from both references.

- page 8576, equation (2): The biomass burning emissions in each grid box have a diurnal cycle based on r(t). This is a particular feature of the presented emission inventory and could be highlighted more, e.g., by adding '(t)' on the left hand side of equation (2).

(A) Done.

- page 8576, line 4: Add the local time of the maximum fire activity.

(A) Done.

- page 8576, line 16: The Duncan et al., 2003, paper refers to a method to calculate the interannual and seasonal variability of biomass burning emissions based on satellite observations using a climatological inventory of these emissions as the baseline. Did you use this method to estimate the biomass burning emissions for 2004, or did you use their climatological inventory? Please specify. In the latter case, you should also refer to the original reference for the climatology (maybe Lobert et al., JGR, 1999, is best suited)

(A) We actually tried to use year-specific inventories. The authors did request the Duncan et al. (2003) inventory for the year 2002, however we did not have a positive answer. The reference Lobert et al. (1999) was included.

- page 8576, line 23ff: Please define the regions, where you are comparing the emission inventories, i.e., deforestation areas, cerrado (savanna) areas, more clearly (e.g.,

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using lat/lon information) for the readers who are not so familiar with the typical fire regions in South America.

(A) Done.

- Section 3, 2nd paragraph: The description of the atmospheric model should be shortened, in particular equation (3) and the corresponding text (starting at 'The general mass ....and finishing at ... the plume rise mechanism) should be removed. Details on the model equation are presented and discussed in detail in the accompanying paper by Freitas et al. and do not need to be repeated here.

(A) Done.

- page 8579, line 10: More information on the measurement side in Rondonia is given in Andreae et al., JGR, 2002, and Trebs at al., ACP, 2004. These references should be included here instead of Andreae et al., 2004.

(A) Trebs et al., (2004) was included.

- Section 3.1: It would be helpful to set the 2002 fire season into the climatological context. A recent article (Yokelson et al., ACPD, 2007, page 6925, line 4) suggests that the number of fire hot spots in the 2002 fire season was above average.

(A) The following text is now included: 'The number of fires detected and consequentially the atmospheric smoke loading in Amazonia typically show substantial interannual variability, with a clear positive trend between 2000 and 2005 (Koren et al., 2007, Schroeder et al., 2009, Koren et al. 2009). According to Koren et al. (2007), the number of fires doubled and the smoke loading increased by 60% during this period. The fire activities underwent a strong decrease in 2006, to recover again in 2007 (Schroeder et al., 2009). Actually, the fire incidence in Amazonia is directly associated with deforestation activities (not shown). Therefore, 2002 can be considered a typical year concerning fire activities in Amazonia, and the measurements and simulations presented here should be very representative of the climatological pattern.'

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- page 8579, line 22: add it between 'Also'; and 'is' such that the sentence reads: 'Also it is evident....'

(A) Done.

- page 8579, line22ff: From Figure 4 it is not clear how you conclude that 'the strength of emissions of EDGAR and D2003 for October is too weak'. Between 10 and 24 October the model results based on EDGAR and D2003 seem to closely follow the observations. The underestimation by about 100 ppb could be attributed to the neglect of additional CO emission sources other than biomass burning. It seems that the EDGAR and D2003 emission inventories have the most difficulties in reproducing the very high CO values in September and the beginning and the end of October. Please specify.

(A) A background of 100 ppb is added to the tracers, so our conclusion seems to be correct.

- Figure 4: Please motivate the comparison between instantaneous values from the model simulations and daily-average values from the observations? It seems that the inclusion of the diurnal cycle of the fire emissions (at least in the 3BEM inventory) allows to compare the model more directly with the observations (e.g., on an hourly basis). I suggest using daily-average values for the model and the observations for the intercomparison. This might also reduce the underestimation of the CO mixing ratio during the very polluted period in September, since the CO mixing ratio in the model at 12 UTC is most likely systematically lower than the average CO mixing ratio for that day, which includes the afternoon maximum of the fire emissions.

(A) Model comparison with the surface observations cannot be done on an hourly basis due the mismatch between the scales represented. Our model results are volume averages over aprox. 35 km x 35 km x 100 m (for the surface grid boxes). As demonstrated by the following figure, the maximum values of the CO mixing ratio occur at 12 UTC justifying the way we compare the values.

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Figure 1: Diurnal cycle of CO mixing ratio (ppb) observed during the SMOCC field campaign (black color).

- Figure 4: Since the diurnal cycle of fire activity is included in the emissions is would be very interesting to evaluate the simulated diurnal cycle of the CO mixing ratios with the observations. Is the maximum of the fire intensity at 17:45 UTC reflected in the diurnal cycle of the CO mixing ratio in the observations and/or in the model? Such a comparison has not been presented in Freitas et al., 2007, and certainly would increase the scientific content of the present manuscript.

(A) We included the figure above and a discussion (see section 3.1).

- page 8580, line 9: Refer to Figure 13 from Freitas et al., 2007, as the comparable figure to Figure 4 of the current manuscript for the model simulations including the plume-rise mechanism.

(A) Done.

- page 8580, line 16: add some information on the dates of the flights: '... observations for sixteen flights between ... and ....'

(A) Done.

- Figure 5: Please indicate the date of the flights in the figures, maybe instead of the label 'SMOCC flight' the corresponding date could be included.

(A) Done.

- Figure 6: Please specify, how the mean of the model results was calculated? Could one also add a STD for the model results to the mean mixing ratio?

(A) Model mean is simply arithmetic mean.

- page 8581, line 16: For the intercomparison between the model results and the MOPITT observations, model results from the 'large scale grid&' are used. Is this the grid

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with the horizontal resolution of 140 km? How do the model results compare to those obtained on the high-resolution model grid? Maybe add a figure from the high resolution grid, or at least a statement on the impact of the grid resolution on the model result for these monthly averages.

(A) This information is not correct. We actually show a composite using the high-resolution model grid data over the low resolution one.

- page 8581, line 17: The model results are compared with MOPITT observations for August and October. Since the highest CO mixing ratios were found in September (Figure 4), the model results should be evaluated with MOPITT measurements also for September, as has been done in Freitas et al., 2007. It might be even more appropriate for this manuscript to use the 3BEM simulations as a reference and compare the results obtained using the other biomass burning emission scenarios with those obtained using 3BEM.

(A) Evaluation for September was also performed and the results are similar to the August and October. They are not shown because they did not bring additional information.

- Figure 7: Only relative differences compared to MOPITT CO are presented. To estimate the absolute values of the model errors, please add the corresponding CO mixing ratios derived from MOPITT.

(A) We would not want to include more figures in the paper since it already has 11.

- page 8582, line 3: Where are you referring to when stating that 'For August, 3BEM model errors are smaller at all levels while EDGAR and D2003...'. Smaller compared to what?

(A) Our reference is the MOPITT retrieval. We rephrase the sentence, it reads now as: 'Comparing results for August (Fig. 9), 3BEM presents smaller errors relative to the MOPITT retrievals than EDGAR, D2003 and GFEDv2 at all vertical levels.'

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- page 8582, line 4ff: The underestimation of the CO mixing ratio south of 30 deg S seems to be due to the neglect of tracer inflow from the lateral boundaries (see Freitas et al., 2007, Figure 3, for PM2.5). This should be stated more clearly in the text.

(A) Done.

- Besides the comparison of the monthly mean mixing ratios of CO it would be interesting to evaluate the performance of the model and the three biomass burning emission inventories for particular case studies. The advantages of 3BEM compared to the two other biomass burning emission inventories are probably best seen in such intense case studies. One potential case could be the situation on 27 August 2002 presented in Freitas et al., 2007, or the situation between 22 and 29 September as presented in Freitas et al., 2006. In both cases, 3BEM should perform better than the two other biomass burning emission inventories.

(A) Thanks for the suggestion. We will work on these comparisons later.

- page 8583, line 6: What is meant by '...need for inclusion of diurnal and daily variability as ...'? Does this statement refer to the biomass burning emission inventory? If so, I don’t think that it has been shown that the inclusion of the diurnal variability is required. To show this, a model simulation without the diurnal variability of the biomass burning emissions should be conducted and compared to a full simulation.

(A) The new tracer which source is the monthly mean of 3BEM demonstrates our statement.

page 8583, line 8f: Since no 'isolated and particularly intense long-range transport events' have been studied here, it cannot be concluded that daily variability is a requirement for biomass burning emissions for investigations of such events. However, the manuscript would gain substantially if such events would be included and the validity of the statement could be shown.

(A) The sentence was removed.

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