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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) – Part1: Model description and evaluation” *by* S. R. Freitas et al.

S. R. Freitas et al.

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Q)General Comments The manuscript describes an "online" chemical tracer model based on the Regional Atmospheric Modeling System (RAMS). It shows the need of using surface data for soil, vegetation, and emissions in order to improve model performance. The model performance is evaluted using data from the Large Scale Biosphere-Atmosphere Experiment in Amazonia. However, the manuscript is not very well structured and difficult to read. The authors do not make clear what the really new features of the model are and neither do they focus the evaluation on any particular model feature. However, I recognize that the manuscript contains new elements and believe that they are worth being published in ACP after some major comments have been addressed: In the first place, the authors do not make clear what the objective of the manuscript is. From what I understand is that there are some processes responsible for the transport of aerosols and chemical tarcers which are very particular to the South American continent (and here particluarly Brazil): i) surface processes, ii) convection in the tropics and subtropics, iii) interaction of highly absorbing particles with radiation, and iv) plume rise due to vegetation fires. Point iv) has been treated in a different paper (as cited by the authors) and therefore should not (and is not) subject to the manuscript. However, the other three points are vital and are somehow treated in the manuscript, however, without any real focus on them. A) We thank to the reviewer of this manuscript for his/her insightful and helpful comments. The paper is now much improved by his/her comments and corrections. The manuscript had a full and deep revision and re-organization and, now, we understand that our objectives are more clearly stated.

Q)Specific comments With respect to point i) in the general comments, the authors do not really seem to contribute to the model development, but rather use alternative data for for soil properties and vegetation. A) We now stated clearly our contribution. Improved data sets describing surface properties of South America were collected from different authors and implemented in the model, as previously stated on the paper. However the authors implemented biophysical parameters appropriated to the main biomes of South America as well as extended the root distribution of tropical forest to deep soil layers, as observed, improving the surface parameterization for tropical and sub-tropical areas.

Q)With respect to point ii) in the general comments, the authors only demonstrate the impact of the convection scheme on precipitation, as shown in chapter 4. In fact, in chapter 2 (page 8530, first paragraph) the authors state that the implemented convection scheme in BRAMS "improved the simulation of Amazon basin moist convection spatial distribution as well as its temporal occurence". Do the authors refer here to some published work? In this case the manuscript would need a reference and the last two paragraphs of chapter 4 could be taken out. Otherwise, tha above mentioned statement is inappropiate in chapter 2 and the evaluation of convection is one objective of this manuscript and would have to be stated so in the intorduction. A) We now included 2 references where we show that the convection over Amazon is well represented by the features we implemented. We also discussed the three main reasons for the need of new cumulus scheme: 1. the standard RAMS scheme was not based on the mass flux approach and, so, difficult to consistently include the convective transport of tracers; 2. the scheme, based on Kuo (1974) approach has strong coupling with the large scale moisture convergence which is not the main mechanism that generates convection on Amazon forest and cerrado areas during the dry season; 3. this scheme does not account for shallow non-precipitating convection.

Q)Even more important though, the model supposedly contains convective transport of chemical tracers and aerosols, however, the importance of including it is nowhere demonstrated (neither is it clear whether it really has been used in the simulations); this needs an evaluation by means of comparing the demonstrated results with control runs which do not include convective transport.

A) The model evaluation using CO MOPITT data at levels 300 and 250 hPa (section 5.2 and figure 16) shows the importance of including the convective transport (in this case, associated to deep precipitating cumulus). A detailed evaluation is given at section 5.3.1 and figure 17, where we describe the convective transport of CO by deep convection on Amazon. This case represents a situation where convection was triggered on instability areas associated to a cold front approach from the South. To demonstrated the importance to include convective transport, we refers now to a detailed evaluation using controls run which do not include this mechanism published in Freitas et al., 2007 (originally from Freitas et al., 2006, ACPD) Freitas et al.: Including the sub-grid scale plume rise of vegetation fires in low resolution atmospheric transport models, Atmos. Chem. Phys., 7, p. 3385-3398, 2007. Freitas, et al.: Including

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the sub-grid scale plume rise of vegetation fires in low resolution atmospheric transport models, Atmos. Chem. Phys. Discuss., 6, 11,521-11,559, 2006.

Q)With respect to point iii) in the general comments, there is no description whatsoever on how the interaction of aerosol particles and short and long wave radiation is dealt with. Neither is there any evaluation or discussion on the importance and/or impact of including such interaction. In fact, this interaction combined with the particulary great amounts of emissions and long range transport of aerosols described in the manuscript would be the major contribution of this work. Therefore, the way it is dealt with by the authors is highly unsatisfactory.

A) The text “For smoke aerosol, a generic particle (without elemental composition specification) was assumed, with an average mass density of 1.35 g cm-3 (according Reid et al., 1998a) and spectral optical properties following the AERONET Amazonian climatology from Procopio et al. (2003)” was included in the paper e a reference add (Longo et al., 2006B). As recommended by an anonymous reviewer of the companion paper (Longo et al., 2007), the feedback of aerosol simulation on meteorological fields and tracers through radiation parameterization is now included in that paper. See more comments and a short discussion below.

Q)Page 8528, first paragraph: Grell et al., 2000, is also a regional fully coupled "online" transport model. In fact, the development of regional fully coupled "online" transport models started much earlier as the authors suggest (see therefore: Jacobson, M.Z, 2006, Discussion, Atmospheric Environment, 40, 4646-4648). A) The paragraph was reformulated.

Q)Page 8530, first paragraph: What are these "several biophysical parameters"? This is very vague.

A) We rephrase the text, now we specify the parameters: “The biophysical parameters maximum stomatal conductivity, leaf area index, albedo, roughness, biomass and soil heat capacity, soil porosity, hydraulic conductivity and moisture potential at sat-

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uration and root distribution associated with the vegetation and soil parameterizations of RAMS were adapted for tropical and sub-tropical biomes and soils, using observations or estimations obtained in recent field campaigns, mostly associated with the LBA program”

Q)Page 8531, first paragraph: Why is a different advection scheme used for CATT than for scalars in RAMS. In fact, using this different scheme means that the CATT-BRAMS is NOT fully consistent. A) We did not say that tracers use different advection scheme. It’s exactly the same RAMS uses to advect scalars. The sentence was rephrased.

Q)Page 8531, line 9-13: This description is given in chapter 2, and therefore unnecessary here. A) The sentence was deleted.

Q)Page 8531, line 15-17: Reference needed for both resistance and patches approach. A) The references Seinfeld and Pandis (1998) and Walko et al. (2000) were included.

Q)Page 8528, last paragraph: Figure 1 is really not necessary, even more so as the authors point out that its incomplete. A) We want to keep because it’s pedagogical and give a good picture of the main sub-grid processes relevant to the low resolution transport models.

Q)Page 8528, last paragraph: Reference or description needed on how aerosol and short and long wave interaction is dealt with (including optical properties of aerosols). A) A short description and a reference were included as stated before.

Q)Chapter 2, last paragraph: This paragraph does not contribute to this work scientifically and should be taken out.

A) We want to keep it to register the evolution of our modeling system, since we assume that this paper will be itself a reference for some features of BRAMS model.

Q)Chapter 3: This chapter should only be model description. Section 3.1 should be part of the meteorological evaluation (chapter 4). A) Done.

Q)Chapter 3: Which chemical boundary conditions are used? A) The both grids exchange boundary conditions using a 2-way approach which conserves mass. The parent grid (which covers South American and African continents) has constant inflow and radiative b.c. outflow.

Q)Chapter 3, Figure 2: In Figure 2 A not all colours are described. In Figure 2 B the numbers for the colorbar are partially covered by the colorbar itself. We here focus on only the main biomes affected by fires during the dry season.

Q)Page 8533, line 14-16: Not clear how soil temperature was initilized. A) Soil temperature is difficult to be initialized due the lack of data on regional to global scales. The traditional method used by RAMS model assumes that, at the initial time, the entire soil layer has the same temperature as the air in the 1st model layer above the surface.

Q)Page 8533, line 24-28: Do the authors refer to some previous work? In this case a reference is needed. Otherwise this statement is inappropiate in this chapter and an evaluation of the impact of the use of the mentioned data would have to be shown in A) The sentence was deleted.

Q)chapter 4. Page 8534, line 10-20: This paragraph should only describe the domain locations in detail. Other details of the given figure are not relevant here.

A) We understand this comment is necessary because we also introduces to the readers the general patterns of model output and provide a link to a movie as a supplemental material.

Q)Chapter 4, general: This chapter should focus on the meteorological aspects including the climatology from section 3.1 (much briefer though). A) As recommended by the reviewer, the section 3.1 belongs now to the chapter 4.

Q)Chapter 4,fFigure 5: It is not clear where the observations come from, where they are located, and whether they are considered representative. A) Both surface fluxes measurements have references (von Randow et al., 2004 and Miranda et al., 1997) and

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they are considered as representative of these areas during the dry season. Anyway, to the best of our knowledge there are not other sources of data available for those biomes.

Q)Chapter 4, first paragraph and Figure 7: Figure 7 shows the PBL height, however no real impact (as stated in the text) is shown. In order to show an impact the authors would need to have a control run and the results which they show in the manuscript.

A) We agree that showing the impact of this on model simulation would be important. However, including figures and detailed discussion about this would increase the length of the paper. In the current version, we just want to show that the model is able to simulate coherently the typical mixing layer height for the main biomes, if the main surfaces processes are properly represented. Q)Chapter 4, second and third paragraph: Both paragraphs present a more general meterological evaluation of RAMS(BRAMS) in Brazil. Again the focus of the evaluation here should be on the impact of new features in BRAMS on these results. A) On section 2, we now discuss how a not consistent simulation of the surface fluxes affected the tracer simulation in previous work (Freitas, 1999). Here we want to emphasizes the capacity of BRAMS simulates realistically the Bowen ratio of the main biomes affected by fires in South America. We assumed that this first step is an essential one to assess the tracer’s transport simulation within any framework: ‘on-line’ or ‘off-line’. Q)Chapter 4, figure 8: Why do the authors compare temperature and not potential temperature? A) We now compare potential temperature.

Q)Chapter 5, general: This chapter is a straight comparison of model results and observations. It does not evaluate the importance of an "online" model for the realistic simulation of CO and PM2.5 transport (and that is what the authors want to show, I suppose), including feedback mechanisms between aerosol concentrations and radiation. This evaluation is necessary here.

A) We did not advocate that we need an ‘online’ models for realistic

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simulation of the transport of tracers. The two approaches, on-line or off-line, has advantages and disadvantages and can be used if they provide realistic dynamics (specially, PBL development and clouds transport). In this paper we just want to show our model evaluation against near surface and airborne measurements and remote sensing derived data. As recommended by an anonymous reviewer of the companion paper (Longo et al., 2007), the feedback of aerosol simulation on meteorological fields and tracers through interaction with radiation is now included in that paper. A short discussion is provided here. The figures below shows the differences on the CO simulation taking in account the radiative effect of biomass burning aerosol (AER ON) or not (AER OFF). Figure 4 (See pdf file uploaded at COSIS web site) shows a comparison between the mean CO from LBA-SMOCC/RaCCI field and the mean of model results with the aerosol radiative effect and without. As can be noted, the non-inclusion of the aerosols causes a slightly difference between the two simulated profiles. Not including the aerosols, the atmosphere is more unstable and favors the upward vertical distribution of CO.

The Figure 5 (See pdf file uploaded at COSIS web site) reinforces the discussion above. The CO distribution is showed now as a monthly and areal means from surface do 13 km height. September and October are showed (August did not presented significant differences). For September, the differences are still small but distinguishable. October presents noticeably differences. Note that the biggest difference appears just above the surface, as expected, due the trapping effect of aerosol stabilization. The difference between the simulations is around 30 ppb (˜ 10 % of the amount). In the PBL (first 1500 m), there are smaller differences around 15 ppb. Note that the blue line has stronger gradient from 1500 to 2500 m than the red one, denoting less vertical mixing. Around 3000 m the situation is inverted, with greater amount of CO in the simulation without aerosols. In the low to middle troposphere (2500 to 7000 m) the differences are much smaller and indicate that the aerosol does not significantly affect the plume rise mechanism, an important transport process affecting these levels as showed by Freitas et al. (2006 at ACPD, 2007 at ACP). In the upper atmosphere, the differences become noticeable again (maximum absolute value around 10 ppb) and indicates the effect of convective inhibition caused by the aerosol stabilization. Less deep convective systems are simulated in the case where aerosol is included which will, of course, impacts the convective transport of CO from surface to upper levels.

Q)Chapter 5: STD seems never being identified as standard deviation. A) It was first defined at Section 4.2 (page 8537, line 15).

Q)Section 5.1, first paragraph: Is this really R2 (coefficient of determination) or is it rather R (correlation coefficient)? A) This is coefficient of determination. It is corrected now.

Q)Section 5.1, las paragraph: I think the authors miss the point here. Off course it is difficult to "simulate observed profiles associated with biomass burning". But the authors seem to have done rather well in doing so. Therefore, they should in fact show the importance of doing so. Particulary, what happens if we do not include interaction aerosols/radiation and improved surface fluxes within the meteorology to the transport of trace gasses and aerosols? This is very important since this would really justify the use of an "online" model. However, the authors do not elaborate on their real contribution.

A) Please, see answer for “Chapter 5, general”.

Q)Section 5.2, first paragraph: A positive model error should mean an over- estimation, a negative a sub-estimation, and not vice versa as it is using the author’s definition of model error. A) Maybe our definition is not the best one available. However, following the way we did, an over-estimation means COMODEL> COMOPITT and, so, implies on negative value for ME. Q)Section 5.2, last paragraph: Figure 16 shows that the model gives good results. However, it is not demonstrated that this is "mainly a result of the improved deep moist convection and plume rise parameterization", as the authors state. A) We rephrase the sentence. It now reads: “As pointed out by Freitas et al. (2007), the transport of biomass burning CO to the upper troposphere is

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fairly dictated by deep convection and plume rise mechanism. In that way, the model results demonstrate the reliability of the numerical parameterizations of the former processes included in the modeling system.” Q)Chapter 6: The discussion should be focused on the new features in the model and their impact on model results. A) This section had a full revision and now we understand that it reflects better the new features and their impacts.

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