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Atmospheric Chemistry and Physics Discussions

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.

Anonymous Referee #2

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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.

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.

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. 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

ACPD

7, S2963–S2968, 2007

by means of comparing the demonstrated results with control runs which do not include convective transport.

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.

In summary, the authors should focus on the important features they want to show, state that in the introduction and structure the manuscript accordingly.

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).

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

Page 8531, first paragraph: Why is a different advection scheme used for CATT than for sclaras in RAMS. In fact, using this different scheme means that the CATT-BRAMS is NOT fully consistent.

Page 8531, line 9-13: This description is given in chapter 2, and therefore unnecessary here.

Page 8531, line 15-17: Reference needed for both resistance and patches approach.

Page 8528, last paragraph: Figure 1 is really not necessary, even more so as the authors point out that its incomplete.

ACPD

7, S2963–S2968, 2007

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).

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

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

Chapter 3: Which chemical boundary conditions are used?

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.

Page 8533, line 14-16: Not clear how soil temperature was initilized.

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 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.

Chapter 4, general: This chapter should focus on the meteorological aspects including the climatology from section 3.1 (much briefer though).

Chapter 4,fFigure 5: It is not clear where the observations come from, where they are located, and whether they are considered representative.

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.

Chapter 4, second and third paragraph: Both paragraphs present a more general me-

ACPD

7, S2963–S2968, 2007

terological 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.

Chapter 4, figure 8: Why do the authors compare temperature and not potential temperature?

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.

Chapter 5: STD seems never being identified as standard deviation.

Section 5.1, first paragraph: Is this really R2 (coefficient of determination) or is it rather R (correlation coefficient)?

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.

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.

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.

ACPD

7, S2963–S2968, 2007

Chapter 6: The discussion should be focused on the new features in the model and their impact on model results.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 8525, 2007.