

## ***Interactive comment on “Including the sub-grid scale plume rise of vegetation fires in low resolution atmospheric transport models” by S. R. Freitas et al.***

**S. R. Freitas et al.**

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Response to Anonymous Referee #1 The manuscript by Freitas et al., presents a novel approach to represent the emissions of vegetation fires by including a one-dimensional plume rise model in a regional model. The vertical displacement of fire emissions due to the heat emitted from fires is often neglected or very simplified considered in larger scale models. By explicitly considering the thermodynamical state of the atmosphere and the heat emitted by the fire, the current approach presents a first step towards a more process-oriented description of the injection height of fire emissions in larger-scale models. The manuscript is well written and contains a good description of the underlying equations of the one-dimensional plume rise model, its implementation into a regional model, and first model results that show and evaluate the impact of con-

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sidering the injection height in the regional model. Enclosed are my specific comments that should be considered before publication of the manuscript in ACP.

We thank the referee for his/her kind words, the replies (A) to the specific comments (Q) are given below.

Specific Comments: Q1) page 11523, line 23: the reference Wang et al., 2005 should read Wang et al., 2006.

A1) Done.

Q2) page 11526, line 8: It is stated that the final height that the plume reaches is controlled by the thermodynamic stability of the atmospheric environment and the surface heat flux release from the fire.. While I generally agree with this statement, I am wondering if horizontal wind might also contribute to the final plume height. Especially for small fires, the horizontal wind might prevent the fire plume from reaching the condensation level and/or enhance the entrainment. Both aspects should lead to a lower height of the plume. On the other hand, the fire itself probably will intensify at higher surface winds. It might be worth to include a brief statement on the possible impact of horizontal wind on the resulting injection height.

A2) Yes, you are right. The following statement was added in the text and situation description is now much more complete: “Also, in presence of strong horizontal wind, it might enhance the lateral entrainment and even prevent the plume to reach the condensation level, particularly for small fires, impacting the injection height.”

Q3) Section 2: It might be easier to follow, if the equations 1 to 5 would be presented before the explanation of the variables.

A3) Thanks for the suggestion. The system of equations appears now before the explanation of its variables.

Q4) page 11527, line 4: please specify the definition of the buoyancy term B (does it include g?) A4) B is related to the difference of temperature between the in-cloud air

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parcel and its environment and includes the downward drag of condensate water. This sentence is now included in the text. The gravity accel. 'g' is not included in B.

Q5)page 11530, line 1 ff: Please include a reference for the emission factor of water vapor, and the rate by which biomass is consumed.

A5) Done: Byram, G. M.: Combustion of forest fires, in: Forest Fire Control and Use, edited by Davis, K. P., McGraw-Hill, New York, 1959. Alexander, M. E.: Calculating and interpreting forest fires intensities, Can. J. Bot., 60, 349-357, 1982.

Q6) page 11531, lines 15 ff, Fig. 2: The two radiosonde profiles have very similar surface properties (temperature slightly above 30 deg C, and dew point slightly below 20 deg C). I am wondering if the discrimination of the two radiosonde profiles based on their humidity (dry and wet) is appropriate here, since the evolution of the ascending parcel is highly determined by the initial (i.e., the surface) properties. Only entrainment at higher levels leads to differences in the water content of the rising plume between these two cases. The differences between the plume rise calculated for the two atmospheric profiles are (correctly) explained by the different temperature profiles and not so much by the difference in the humidity. This might be worth mentioning.

A6) Thanks for call our attention to this point. Since our study focus on the dry season, was no possible to find a true 'wet' situation. So, the names gave to the two situations 'dry' and 'wet' was determined from the moist situation of atmosphere above. However, your point is really important and needs to be included in the text. The original sentence: 'In the "wet" case, the total condensate water is greater and generates positive buoyancy acceleration, which does not occur in the "dry" case. This imposes a higher plume rise (by about 500 m) above the inversion for the wet case.' Was replaced by: 'In the "wet" case, the total condensate water is greater, as the environmental air entrained by the lateral eddies are much moister, and generates positive buoyancy acceleration, which does not occur in the "dry" case. This imposes a higher plume rise (by about 500 m) above the inversion for the wet case.'

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Q7) page 11531, line 24: It might be worth including the values of the initial conditions for the vertical velocity ( $w_0$ ), the density ( $\rho_0$ ) and the temperature ( $T_0$ ) in the rising parcel for this case.

A7) Done.

Q8) page 11534, line 19: please explain what is meant by the 4DDA technique, maybe include a reference.

A8) 4DDA stands for 4-dimensional data assimilation and refers to how the model fields can be nudged toward observational data as a simulation progresses. The reference is: Davies HC (1983) Limitations of some common lateral boundary schemes used in regional NWP models. Mon. Wea. Rev. 111: 1002-1012. These informations are now included in the text.

Q9) page 11534, line 19: how are the background values of CO initialized in the model?

A9) In the absence of data to provide initial condition for CO, we simply set the initial value as 100 ppb in the PBL and 60 ppb above for the entire horizontal domain and run the model for ~15 days with CO sources before the period of interest.

Q10) page 11534, line 25: some more information on the BFEMO would be useful, e.g., what are the underlying assumptions (e.g., emission factors, biomass density, combustion factors)? The numerical values are given in Freitas et al., 2005, as stated, but it might be insightful to remind the reader that some basic assumptions are required for the calculation of fire emissions from satellite derived fire.

A10) The following sentence was included in the text: BFEMO emission approach uses detailed information about emission factors, aboveground biomass density and combustion factors for South America biomes as well as fire counts, derived by remote sensing, to determine location and timing of emission. One basic approximation assumed is the fire size retrieved by remote sensing as burnt area to provide an estimation of the amount of biomass consumed by the fire.

Q11)page 11536, line 13, Figure 8: Is the plume model also used for the injection of the African fire emissions? How are the emissions calculated (it seems that the BFEMO emission model focuses on Brazilian emissions)? Are the underlying assumptions also valid for Africa? Is data from ABBA also available for Africa?

A11) Yes, the plume model is used and BFEMO methodology is also applied for Africa. BFEMO was developed focusing on and using detailed information of South America, however its underlying assumptions are generally valid and it could be extended to others regions if detailed information of emission factors, biomass density, combustion factors and fire count (location, timing, diurnal cycle and estimate of fire size) are provided. Unfortunately, ABBA data is provided only for the American continent.

Q12) page 11536, line 11 ff, Figure 8: It is stated that the fire in Africa are mostly due to savanna fires that produce a broader and lower injection layer. However, the injection height over Africa reaches above 6 km, which seems to be rather unusual for savanna fires. Please comment on the high values for the plume rise calculated over Africa.

A12) Yes, we agree that the simulated injection layer for savanna fires seems to be too high. However, this is mainly related to the lack of information on the fire size as we don't have this kind of information for Africa like we have for South America.

Q13)page 11537, line 14 ff: The motivation for the selection of the different CO tracers is not convincing to me. It is rather obvious that transport of CO that takes only into account advection and PBL diffusion (COAD) is inaccurate. It still is interesting to investigate the impact of no convection (COAD), shallow convection (COSH) and deep convection (CODP) on the transport of CO, but it seems that this is not the main focus of this paper. The main focus of this paper is the impact of including the plume rise (COALL) on the model results. Unfortunately for the comparisons of the model results with observations (5.1., 5.2) the model tracer CONOPR is not presented, even though it is discussed before (page 11534, line 21). This tracer seems to be best suited for an evaluation of the impact of the plume rise mechanism. I suggest to include the

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CONOPR tracer into these intercomparisons and maybe remove some other tracer (e.g., COAD).

A13) Yes, the main focus of this paper is role of plume rise mechanism, however we found be valuable discuss also the relative role of the others convective processes in the same context. COAD is actually inaccurate but we would like to show it because still have transport models that only accounts for advection and PBL diffusion. Anyway, we did not include CONOPR because it is similar do CODP, since shallow convection has only a minor effect at all (see figure 13).

Q14)page 11538, line 15f: Why was the boundary layer development suppressed over regions of dense smoke? Is this a radiative effect? Is this effect included in the model simulation? Please comment and maybe add an appropriate reference.

A14) This is associated to the absorption and scattering of solar radiation by the smoke causing a warming at the top of the layer and cooling at the surface. As a consequence, the atmospheric stability is increased and inhibits the boundary layer growing and mixing. This is a hypothesis that we raise up to explain the situation, however the sampling condition might be also considered. The discussion now includes also this sentence. Yes, our model system account for this process. The following reference is now included: Longo, K. M., Freitas, S. R., Silva Dias, M., Silva Dias, P.: Numerical modelling of the biomass-burning aerosol direct radiative effects on the thermodynamics structure of the atmosphere and convective precipitation. In: International Conference on Southern Hemisphere Meteorology and Oceanography (ICSHMO), 8., 2006, Foz do Iguaçu. Proceedings... São José dos Campos: INPE, p. 283-289. CD-ROM. ISBN 85-17-00023-4, 2006.

Q15) page 11539, line 17f, Figure 13: Please explain (and/or give a reference) how the model results were modified in Figure 13a. What is meant by applying the averaging kernel and a priori data < 50 %? In particular I am confused that at 850 hPa tracer COALL has the lowest CO mixing ratio of all model simulations before the modifi-

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cation (Figure 13b), but the highest mixing ratio after the modification (13a). Please explain.

A15) The modification showed in Fig 13A results from the methodology that has to be applied to the model fields in order to make consistent comparison with MOPITT data according to Deeter et al. (2003). First of all, note that the horizontal scale is different, second, is expected that COALL presents the lowest CO since it includes all PBL venting mechanisms. The fact that it has the highest mixing ratio in Fig 13A results simply from the MOPITT comparison methodology. The important point here is that the model COALL agrees very well with the MOPITT retrieved data. The reference below is now included in the text: Deeter, M. N., et al.: Operational carbon monoxide retrieval algorithm and selected results for the MOPITT instrument, *J. Geophys. Res.*, 108(D14), 4399, doi:10.1029/2002JD003186, 2003.

Q16) page 11539, line 22, 23: It is not clear where the (A) and (B) is referring to, please specify.

A16) The associated figure number was included in the text.

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Interactive comment on *Atmos. Chem. Phys. Discuss.*, 6, 11521, 2006.

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