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Atmospheric
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Interactive comment on "Modeling of biomass smoke injection into the lower stratosphere by a large forest fire (Part I): reference simulation" *by* J. Trentmann et al.

J. Trentmann et al.

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Reply to Referee 3 (The Reviewers comments are contained in brackets):

[This manuscript presents a cloud-resolving model simulation of the well-documented Chisholm fire pyrocumulus case. The paper is very well written and interesting results are clearly presented. I have only minor comments on the manuscript which might help clarify a few points.]

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

[1. Presumably, with the sounding specified deep convection would not occur without

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the enormous fire heat flux. In other words, with typical sensible and latent heat fluxes on the order of a few hundred W/m2, the model would not produce deep convection. This may be an obvious point, but perhaps it is worth stating.]

We added the following sentence at the end of the third paragraph of Section 4:

'The initial atmospheric profile has some potential for convection (see Sect. 2.2), however, without the heat flux from the fire the model would not produce such a deep convective cloud, given a level of neutral buoyancy of 7.4 km of the initial profile.'

[2. page 6045, lines 1-2: The observational result of a linear correlation between fire intensity and injection height seems, at first glance, to be inconsistent with the finding in this study that condensation and freezing dominate over fire heat in the storm energy budget. Given the latter result, I initially inferred that the fire heat simply acted as a trigger for the storm. This issue is addressed in Part II, but perhaps a brief discussion of why the fire intensity is an important factor controlling injection height could be included in this manuscript.]

Enhanced sensible heat flux increases the low-level updraft, which enhances the low-level convergence of potentially humid air, which is included in the updraft and therefore enhances the release of latent heat. In addition, the entrainment of dry air at mid- and high levels of the convection is reduced, due to the enhanced updraft. This gives a positive feedback between the sensible heat flux from the fire and the release of latent heat from ambient air. In the revised version of the manuscript we added the following paragraph at the end of Section 5:

'It must be noted, however, that a positive feedback exists between the sensible heat flux from the fire and the latent heat relased in the plume (Luderer et al., 2006a). Enhancing the sensible heat flux enhances also the total latent heat release in the plume, probably because of enhanced entrainment of humid air at low levels and reduced entrainment of dry air at higher levels. Even though the latent heat dominates the overall energy budget of the plume, the sensible heat is a critical parameter to determine the evolution of the pyro-convection (Luderer et al., 2006a).'

[3. page 6049, lines 7-9: The statement that MISR's multi-viewing-angle measurements indicate that the Chisholm plume was well into the stratosphere is referenced with a web site. Given the transience of web sites, it would be preferable to include a figure. Perhaps the MISR results will be included in the Rosenfeld et al. (2006) manuscript that is referenced several times. Along those lines, it is difficult to evaluate the observational results that are presented in the Rosenfeld et al. manuscript without access to that manuscript.]

We agree with the reviewer that it is not optimal to refer to a website. The MISR results will not be included in the Rosenfeld et al. (2006) manuscript. They will be included in a manuscript by Fromm et al., which is currently in preparation. Since this manuscript can not be fully cited and evaluated, yet, we keep the link to the website in the manuscript and add a reference to the manuscript in preparation. We think that adding the figure to the present manuscript is not appropriate.

[4. page 6049, lines 25-28: Why is the ECMWF tropopause height given rather than the sounding tropopause height?]

The definition of the tropopause height is critical for the calculation of the transport of smoke into the stratosphere. From radiosonde data, the dynamical tropopause cannot be inferred. In the case studied here, the thermal tropopause following the WMO definition is located above the dynamical tropopause at an elevation of 12.3 km, corresponding to a pressure of 184 hPa. The potential temperature at the level of the thermal tropopause is 345 K. Here we chose to use the dynamical definition of the tropopause (2 PVU), which is more meaningful than the thermal tropopause and commonly used in studies of the troposphere-stratosphere exchange (e.g., Holton et al., 1995, Stohl et al., 2003). We include this information in the revised version of the manuscript. The last sentences of Section 2 now read:

'The thermal tropopause based on the WMO-definition is located at 12.3 km, corresponding to a pressure of 180 hPa and a potential temperature of 345 K. Based on ECMWF analysis data, the dynamical tropopause (PV > 2 PVU) was located at a potential temperature of θ=332 K, corresponding to an altitude of z=11.2 km and a pressure of p=225 hPa. Here, we chose to use the PV-definition of the tropopause, which is more meaningful for studies of the troposphere-stratosphere exchange in mid-latitudes (e.g. Holton et al., 1995, Stohl et al., 2003).'

[5. pages 6050-6051: More detail about the microphysical scheme in the model should be provided. What processes are included? How is ice nucleation treated in the model? The fraction of smoke particles acting as CCN should depend on the assumed smoke size distribution/composition and updraft velocities. More detail should be given describing how the 5% number was determined. Do the smoke particles act as ice nuclei in upper parts of the cloud?]

We agree with the reviewer that the description of the microphysical scheme of the model is rather brief and extended it in the revised version of the manuscript. The section about the microphysical scheme now reads:

'Cloud microphysical processes are simulated using a two-moment scheme that predicts the numbers and mass mixing ratios of four classes of hydrometeors (cloud water, cloud ice, rain, graupel) and water vapour (Textor et al., 2006a). The size distribution of each mode is represented by a generalized gamma function. In total 13 processes that transfer water between the five classes (four classes of hydrometeors and water vapor) are included in the scheme. These include water vapor transfer processes (i.e., condensation at and evaporation of liquid droplets as well as deposition at and sublimation on ice particles) based on the approach by Byers (1965), autoconversion of cloud water/cloud ice into the rain/graupel class, respectively, based on the approach by Murakami (1990), accretion due to a differential fall velocity between different hydrometeor classes (Textor et al., 2006a), and freezing of supercooled water following the stochastic approach of Bigg (1953). Within this approach, commonly used in microphysical parameterizations in convective cloud models (e.g., Reisin et al. (1996);

Seifert and Beheng (2006)), the potential of the smoke particles to act as ice nulcei (IN) is not explicitly taken into account. Observations suggest that smoke particels can act as IN (Hobbs and Locatelli, 1969), but large uncertainties remain. Therefore the stochastic hypothesis for freezing of droplets seems appropriate for the present study. At temperatures below -36 C, homogeneous freezing is considered in the model simulations (Pruppacher and Klett, 1997).

The activation of aerosol particles cannot be treated explicitly in such an approach. Sensitivity studies were conducted using a cloud parcel model with explicit treatment of aerosol activation (Simmel and Wurzler, 2006). The influence of the aerosol number concentration, the aerosol size distribution, the vertical velocity, and the soluble fraction of the aerosol on the fraction of activated aerosol particles has been investigated (Martin Simmel, pers. comm., 2003). In these model simulations, the number of activated smoke particles, i.e., the number of aerosol particles that act as cloud condensation nuclei, was most sensitive to the aerosol number concentration. For the high number concentration typically found in pyro-convection ($> 80\,000\,\mathrm{cm}^{-3}$), only a very small fraction of the aerosol particles becomes activated. Based on the cloud-parcel model results, within ATHAM we assume that 5% of the smoke aerosol particles act as CCN. The exact value of the activated aerosol fraction used here for the conditions in pyro-convection must be considered a rough estimate. However, it was found that the microphysically induced effect of the fire aerosols on dynamics is rather small (Luderer et al., 2006a). ....’

[6. page 6053, 1st paragraph: Is the heat source specified in the simulation moving or stationary?]

The heat source in the simulation is stationary. Sensitivity studies using a moving fire front yielded a low sensitivity of the resulting pyro-convection. We clarified the treatment of the fire fluxes in the manuscript. We added the following sentences at the end of the second paragraph of Section 4.1:

'In the model, the fire fluxes are held constant throughout the simulation. Not enough information on the fire behaviour is available to include a more realistic spatial and temporal distribution of the fire emissions. As part of this study, test simulations using a moving fire front have been conducted (not shown), which showed no impact of the moving fire front on the model results.'

[7. page 6054, line 1: What is the assumed size of the smoke particles?]

We assume a volume mean diameter of the emitted smoke aerosol particles of 300 nm for the young smoke from the Chisholm fire. This number lies within the range of values reported by Reid et al., 2005 for measurements in young smoke plumes. We added the following sentence to the manuscript:

'The particulate emissions from the fire were calculated using the emission factor of 17.6 g kg^{-1} from Andreae and Merlet, 2001 assuming a volume mean diameter of 300 nm (Reid et al., 2005).'

[8. equation 1: The definition of ca should be given immediately after the equation.]

We agree with the reviewer and moved the definition of ca immediately after the equation.

[9. page 6058, lines 8-16: It should be noted that the convective intensity (as indicated by wmax) is showing no signs of dying down by the end of the 40-min simulation. Does the maximum injection height occur earlier in the simulation? It is a little unclear whether the 40-min simulation is long enough to represent the intense convective stage of the storm.]

Since there is a constant heat flux from the fire, the convective intensity is high even at the end of the simulation after 40 minutes. However, the maximum injection height is reached already after about 20 minutes.

[10. page 6059, lines 24-25: The contribution of water vapor from the fire is compared to the total water vapor in the plume. Wouldn't it make more sense to compare it to the total water (including hydrometeors)?]

We thank the referee for spotting this error in the manuscript and apologize for the confusion. We did compare the total water emitted by the fire with the total water in the plume. The sentence now reads:

’Above about 4000m, the contribution of water from the fire is less than 10% of the total water available in the plume.’

[11. references: The Luderer et al. [2006b] paper is missing from the reference list.]

The reference Luderer et al. [2006b] refers to the manuscript in the footnote on page 6055. This footnote now reads:

Luderer, G., Trentmann, J., Hungershöfer K., et al.: The role of small scale processes in troposphere-stratosphere transport by pyro-convection, Atmos. Phys. Chem. Discuss., in preparation, 2006b.

Additional changes:

As a reply to a comment by referee 3 of the companion paper (Luderer et al., 2006a), we added the following text to the third paragraph in Section 4.1:

’Additional uncertainty exists related to the fate of the radiative energy emitted by the fire. In the thermal infrared, where most of the fire radiation is emitted (Wooster, 2002), aerosols are rather inefficient absorbers. It is likely that most of the radiative energy from the fire is absorbed by cloud droplets or gaseous absorption at cloud base or in air masses that are entrained into the convection. In both cases the radiative energy from the fire is trapped in the lower part of the pyro-convection and therefore contributes to the convective energy. Considering these radiative processes in detail is not possible in the present model setup. Here, we assume that all energy released in the combustion process becomes available for convection.’

Additional References:

Bigg, E. K.: The formation of atmospheric ice crystals by the freezing of droplets, Q. J. R. Meteorol. Soc., 79, 107-128, 1953.

Byers, H. R.: Elements of Cloud Physics, The University of Chicago Press, 1965.

Hobbs, P. V. and Locatelli, J. D., Ice nuclei from a natural forest fire, J. Appl. Meteorol., 8, 833-834, 1969.

Holton, J. R., Haynes, P. H., McIntyre, M. E., Douglas, A. R., Rood, R. B., and Pfister, L.: Stratosphere-troposphere exchange, Reviews of Geophysics, 33, 4, 403-439, 1995.

Murakami, M.: Numerical modeling of the dynamical and microphysical evolution of an isolated convective cloud 8722; The 19 July 1981 CCOPE cloud, J. Met. Soc. Japan, 68, 2, 107-128,1990.

Pruppacher, H. R. and Klett, J. D.: Microphysics of Clouds and Precipitation, Kluwer Academics Publishers, 1997.

Reid, J. S., Eck, T. F., Christopher, S. A., Koppmann, R., Dubovik, O., Eleuterio, D. P., Holben, B. N., Reid, E. A., and Zhang,J.: A review of biomass burning emissions part III: intensive optical properties of biomass burning particles, Atmos. Chem. Phys., 5, 827-849, 2005.

Reisin, T., Levin, Z., Tzivion, S.: Rain production in convective clouds as simulated in an axisymmetric model with detailed microphysics. Part1: Description of the model, J. Atmos. Sci., 53, 497-519, 1996.

Seifert, A. and Beheng, K.: A two-moment cloud microphysics parameterization for mixed-phase clouds. Part1: Model description, Meteorol. Atmos. Phys., 92, 45-66, 2006.

Stohl et al.: Stratosphere-troposphere exchange: A review, and what we have learned from STACCATO, J. Geophys. Res., 108, 8516, doi:10.1029/2002JD002490, 2003.

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