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Atmospheric Chemistry and Physics Discussions

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.

Anonymous Referee #3

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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:

1. Presumably, with the sounding specified deep convection would not occur without the enormous fire heat flux. In other words, with typical sensible and latent heat fluxes on the order of a few hundred W/m^2, the model would not produce deep convection. This may be an obvious point, but perhaps it is worth stating.

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.

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.

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

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?

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

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

8. equation 1: The definition of c_a should be given immediately after the equation.

9. page 6058, lines 8–16: It should be noted that the convective intensity (as indi-

ACPD

6, S2651–S2653, 2006

cated by w_{max}) 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.

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)?

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

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 6041, 2006.

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6, S2651–S2653, 2006

Interactive Comment