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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 II): Sensitivity studies" *by* G. Luderer et al.

Anonymous Referee #1

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This manuscript presents a cloud-resolving model results indicating the sensitivity of pyrocumulus injection of smoke into the stratosphere to factors such as fire sensible heat flux, fire latent heat flux, smoke particles acting as CCN, and meteorological conditions. The paper is well written and the results are very interesting. I have several comments on the manuscript that I would like the authors to consider:

1. page 6087: 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 de-

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scribing how the 5% number was determined. Do the smoke particles act as ice nuclei in upper parts of the cloud? TKE should be defined.

2. Several places in the manuscript, the authors state that the Andreae et al. [2004] paper demonstrated that increasing aerosols results in intensification of cumulus convection. However, the observational result in the Andreae et al. paper was that onset of precipitation is delayed when aerosol concentration is enhanced, and they *speculated* that convective intensity at upper levels would be enhanced since more water could reach the freezing level. The distinction between observational results and speculation in the Andreae et al. paper should be made clear. For example, on page 6085, lines 22–26, the authors state that aerosol pollution can significantly enhance vertical development of convection. This statement should be that aerosol pollution *may* enhance vertical development of convection. The following sentence states that Andreae et al. sampled polluted convection that was significantly stronger than convection under clean conditions. In fact, Andreae et al. simply said that they were surprised that the polluted convection was intense given the suppressed surface heating under smoky conditions.

3. page 6093, lines 18–21: The discussion here is confusing. The first sentence compares REF to PRE, then the second sentence compares PRE to REF, using the pronoun "its" to refer to PRE. One or both of these sentences should be revamped.

4. page 6098, lines 16–28: The authors state that the fire moisture has a relatively minor impact on the convection. However, if one assumed that the fire heat lost to radiation were significant (REF assumes no loss, and the authors acknowledge that this the radiative loss is very uncertain), then the impact of fire moisture would be much larger. In the following paragraph, the authors show that fire moisture has a signifant impact on mass of smoke injected into the stratosphere. Perhaps this result should be included in the abstract.

5. page 6101, lines 1–7: The implication here is that in the loCCN case, ice production

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in the updraft is dominated by immersion freezing. However, previous modeling and observational studies (e.g., Heymsfield et al., JAS, 62, 2005) have shown that in convection with relatively strong updrafts ($>$ 5 m/s), heterogeneous nucleation is relatively unimportant and most droplets reach the homogeneous freezing level. As discussed above, the ice nucleation scheme and assumptions need to be described. Again, a key issue here is whether the smoke particles act as ice nuclei.

This may just be an editorial/typeseting issue, but some of the figures were far too small.

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 6081, 2006.