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Atmospheric Chemistry and Physics Discussions

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Interactive Comment

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.

G. Luderer et al.

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Reply to referee #1 (The reviewer's comments are contained in brackets):

[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]

We would like to thank the referee for her/his review and the constructive comments. Replies to the specific comments are given below.

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[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 describing how the 5% number was determined. Do the smoke particles act as ice nuclei in upper parts of the cloud?]

There is a total of 13 processes included describing the transfer of water between the four hydrometeor classes (cloud droplets, rain drops, ice crystals, graupel) and the gas phase, such as growth through condensation and deposition to ice, coagulation within and between the various hydrometeor classes. Heterogeneous freezing is treated according to the stochastic hypothesis (Bigg, 1953). However, like in most other cloud resolving models, there is no explicit treatment of ice nuclei in the model. More detail on the microphysics scheme implemented in ATHAM is also available given in Textor et al. (2006a,b).

The description of the cloud microphysics was extended in Section 2 of this paper and also in the companion paper by Trentmann et al. The number of 5% of aerosol particles activated was adopted from a study with a cloud parcel model. This is now also described in further detail in Trentmann et al. In Section 4 and the summary it was noted that the model does not account for the possible effect of the ice nucleating ability of smoke.

[TKE should be defined.]

A more detailed description of the turbulent kinetic energy (TKE) approach used was added to the text.

[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

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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.]

At several places in Sections 3.3 and 4.4 the wording was changed in order to accommodate the concerns stated by the reviewer.

[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.]

The wording of the two sentences was changed to avoid confusion.

[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 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 significant impact on mass of smoke injected into the stratosphere. Perhaps this result should be included in the abstract.]

We also conducted a sensitivity study without moisture release from the fire and reduced sensible heat flux (SH50/noH2O). Indeed the difference in injection height between this model run and the model run with reduced sensible heat flux and moisture flux from the fire is greater than the difference between the REF and noH2O runs pre-

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sented in the paper. Also in the case with reduced sensible heat flux, however, the effect of removing the fire moisture is much smaller than the effect of changing sensible heat flux from 100 % to 50 %. Since in both in the SH50 and the SH50/noH2O runs there is almost no significant stratospheric injection, SH50/noH2O was not included into the manuscript.

The result that fire moisture enhances the mass of smoke injected into the stratosphere has been added to the abstract.

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

Heymsfield et al. (2005) found indeed that at a level slightly below the homogeneous freezing level within a certain region of the updraft core the number distribution is dominated by liquid droplets that have not been subject to heterogeneous freezing processes. The cloud water content, i.e. the mass, by contrast, is dominated by heterogeneously frozen particles. Another key finding was the important role of very small ice crystals originating from homogeneously frozen cloud droplets that formed on mid-tropospheric CCN that have been entrained laterally into the convective cloud.

Our findings and those of Heymsfield et al. can be reconciled by considering that

1)The existence of free-tropospheric CCN was not considered in our model, therefore the mechanism described by Heymsfield et al. for the formation of small ice crystals from droplets that formed on laterally entrained CCN is not represented in our simulations

2) According to the stochastic hypothesis, immersion freezing is highly size dependant. The droplet sizes in the relevant updraft regions are much smaller in the case studied by Heymsfield et al. than in the loCCN case presented here. Where liquid water was observed by Heymsfield et al., the size distribution peaks in the diameter $<$10 μm bin, and the mean diameter is about 15-20 μm. By contrast, the volume mean diameter in our simulation was about 30 μm for the loCCN case.

3) Both our simulation and Heymsfield et al.'s observations indicate that heterogeneous freezing accounts for most of the mass of the ice produced. The ice water content is the quantity determining the amount of latent heat released and hence the key driver for the effects on the convection dynamics.

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

The figures will be printed larger in the final ACP layout in A4 format.

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

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