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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Aerosol effects on clouds and precipitation during the 1997 smoke episode in Indonesia" *by* H.-F. Graf et al.

H.-F. Graf et al.

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First we would like to thank both reviewers for constructive remarks. We revised the manuscript according to these and hope that it now is in a fairly improved shape.

Specifically, we included some extra lines explaining the technique of the CCFM, thus avoiding that the reader has to refer to our previous papers in ACP.

Referee 2 asks for in-depth statistical analysis against the model internal variability and that we compare our simple internal 1D cloud model against a cloud-resolving model.

In our case, we suppress model internal variability quite substantially due to forcing the model boundary every six hours by ERA-40 reanalysis data. Model internal variability therefore is small and is produced mainly by the cloud effects and the additional aerosol. Hence, what we see as differences between the control and the experiment

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run is the effect of aerosol, in good part modulated by the aerosol-microphysics-cloud effects. In-depth statistical analysis must be based on much extended runs including a larger number of meteorological situations. This is what we propose in the discussion to do next, but this kind of analysis requires a lot of additional work. Our current study serves, as we believe, as a step towards this goal and hopefully also facilitates receiving the necessary funding by showing the feasibility of such investigation.

It is also clear from the beginning that a simple entraining parcel model as we use it cannot compete with a truly cloud resolving model (where one would have to discuss which kind of resolution that must have; we would argue this should be in the order of less than few 100m in order to resolve the in-cloud dynamics relevant for cloud microphysics). Well known bulk parameterizations as used here, and in most sophisticated climate models today, have their deficits and we are aware of those and mention them. However, climate modeling always must make compromises and our aim was to show that we can, better than with the common standard approach, improve simulation quality and even can, limited as we commit, include aerosol effects on convective clouds. Hence we find it undue to perform extended comparisons against cloud resolving models at this stage.

Specific remarks: Reviewer 1: p. 17101, l. 20 Please give some more explanation about what you mean with several different initial radii.

We included specific numbers and one example.

p. 17102, l. 3 The convective model should be explained (here or at the beginning of p. 17103) with one-two more sentences, since it is a central aspect of the article. Please explain the most important aspects of the new convective cloud filed model.

Done so.

p. 17102, l. 13 Please write that you use the ERA-40 dataset and eventually give a reference (e. g. Uppala et al., 2005, Quarterly Journal of the Royal Meteorological

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Society 131: 2961-3012).

Done so.

p. 17102, l. 23 These weekly ...: Please review this sentence to better connect it in the previous part of the paragraph.

Done so.

p. 17103, l. 1 As before: please explain in more detail the main aspects of the CCFM and the cloud model.

Done so.

p. 17103, l. 9 Could you please give some more information about the division in the three cloud types (typical values?).

Done so.

p. 17103, l. 15 Please explain in more detail why it allows for an intensification of precipitation in polluted areas....

Included one explanatory sentence.

p. 17103, l. 19 Why do you used the formula of Berry instead of Kessler? Short explanation of the advantages?

Done so.

p. 17104, l. 1 As before: why do you used the formula of Beheng instead of Sundquist?

Is now explained shortly.

p. 17104, l. 24 How big is the sensitivity of the results if you suppose that the aged aerosol are not such effective as cloud condensation nuclei as you assume?

That very much depends on the differences. However, the parameterizations are not very well constrained anyway (see the quite diffuse cloud of points from which the

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Boucher and Lohmann (1995) parameterization was developed). Truly cloud resolving model simulations would be necessary to answer this question, but such kind of analysis is far beyond the scope of our study.

p. 17105, l. 17 The last sentence of the chapter is not well embedded in the text. Please change the position.

Done so.

p. 17106, l. 7 Please give a short information about the geographical position of the mountains (Borneo-Malaysia).

Included rough model topography in the Figure.

p. 17106 Fig. 1 and 2: I have the impression that the choice of the scale for the figures should be different. Would eventually be better to have shorter distances in the lower part (0-120 cm/month) of the scale and bigger in the higher parts? I have also some difficulties to see the improvement of the simulated precipitation compared to the observation. Only over Borneo it is easy to see it. Eventually could you try to plot Fig. 2 a,b,d,e,g,h as a difference from Fig.1?

We decreased the increment accordingly. Strong effects can indeed only be seen where the aerosol load is high, i.e. over Borneo and South Sumatra.

p. 17107, l. 1 Could you please write an explanation why are they found to the North of the highly polluted area?

This is downwind from the pollution, mentioned in text.

p. 17107, l. 4-19 This part has a repetition of the same concept after few lines. Would it eventually be better to change the text to avoid it?

Rewritten.

Technical Comments ========== p. 17109, l. 14 Fig. 4 instead of Fig. 5.

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Interactive Comment

Changed. p. 17114 Caption: please delete one of the two simulated by REMOTE-CCFM;

Changed.

p. 17116 Fig. 4 has too low resolution

New Figure included.

Referee 2: Specific Comments Page 17101, line 25: "there is still enough (order of 50 mm per month) to allow for the investigation", it is hard to understand what the authors were trying to express here.

We deleted this sentence.

Page 17102, line 14: "the total particulate matter", please describe what is the model prognostic variable, mass mixing ratio or number concentration of aerosols, or both?

Mass mixing ratio, now mentioned in text.

Page 17103, line 7-11: "... three cloud types ... a modified cloud microphysics scheme was used ...", the authors should clearly define the three cloud types and also describe the modified along with the original microphysics scheme. The reader needs to know whether the model is capable in handling the aerosol-cloud interaction issue.

We included the requested information now.

Line 25: "no cm-3", what does this mean?

Simply number per cubic centimeter.

Page 17104, line 9-10: "However, the impact of CCN on the cloud droplet number concentration is not well known", this statement is not accurate. I guess what the authors meant here is that the relationship between aerosol mass and CDNC adopted in many (climate or global) models is rather arbitrary. The reason for such a relationship to exist is that many models only predict the mass but number concentration of aerosols.

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This is correct; we changed text slightly to make clear that the physico-chemical properties, unknown in simple models like REMOTE, are missing. Even if we had the number concentrations, we would not yet know about the potential of the particles to be activated.

Page 17106, line 4-8: "The main precipitation ... any observations", these two sentences could be rewritten.

Done so.

Line 9: "The inclusion of TPM in the model simulation changes the microphysical structure ...", the statistical significance of the results should be provided, and the same applies to several later discussions as well.

As mentioned above (general comments) the internal variability is suppressed by forcing the model every six hours with ERA-40 data.

Line 19: The distribution of aerosol loading should be shown here.

We include one additional Figure.

Line 23 and also Eq. (3): was CDNC distributed uniformly throughout the entire cloud?

No, we corrected text.

Page 17107, line 4: "particle column concentration", please define the particle here. It is the mass-mixing ratio of smoke particles.

Included in text now.

Line 11-16: Did the authors suggest that the fact that the model appears to be too wet has nothing to do with dynamics but aerosol loading? In addition, the CDNC values used in the control run (Page 17105, line 5) seem low, what if higher values (certainly lower than the smoke loading) were used?

No, it seems that the model is too wet due to too high evaporation rate from the warm

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sea and this is a general feature of the REMO family in the tropics. We used the standard CDNC values of REMO. Increasing this would certainly have an effect on clouds and dynamics in the standard model and we discuss this, mainly in terms of the missing sea salt. Currently, a new REMO version is being prepared that contains the relevant aerosol information and this will be used in our follow-up studies.

Page 17108, line 3-4: "few 100 m", please be precise.

Done.

Line 23: "when CAPE is strongly increased in the polluted case" and a few later sentences seem leaving the reader with an impression that the authors suggested that aerosol loading might affect CAPE. Please clarify the sentences.

Indeed, if one looks at hourly intervals, weather as measured by CAPE, may sometimes vary between the control and test runs. This is due to the aerosol and cloud radiative effects, but is confined to very short time scales.

Page 17109, line 14-21: These sentences appear to be not consistent.

We now include a higher resolution figure. Indeed there is weak overall negative radiation effect although at places we find strong and also positive effects in TOA radiation.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 17099, 2007.

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