Geosci. Model Dev. Discuss., 4, C1722–C1734, 2012
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Geoscientific
Model Development
Discussions

Interactive comment on "Evaluation of the sectional aerosol microphysics module SALSA implementation in ECHAM5-HAM aerosol-climate model" *by* T. Bergman et al.

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Received and published: 21 March 2012

General comments In this study an aerosol microphysics model, SALSA, is incorporated into a global aerosol-climate model, ECHAM5-HAM. This manuscript focuses on comparisons of simulated aerosol transport cycle and size distributions with those from observations as well as model description. This model also have the potential to simulate cloud droplet nucleation explicitly. I suggest that this manuscript will be able to be published if the authors fully address specific comments indicted below. Specific comments

We thank the reviewer for valuable comments for improving our manuscript. Reviewers

comments are marked with boldface and after each comment there is our reply.

Attached figures correspond to figures in the manuscript as follows:
Figure 1: Figure 1
Figure 2: Figure 3
Figure 3: Figure 5
Figure 4: Figure 8

P3624, L7-8: "The aerosol size distribution is described using 20 size sections with 10 size sections in size space". It can be understood by seeing Fig. 1. However it is difficult to understand this sentence only in Abstract.

The sentence has been rephrased.

The aerosol size distribution is described using 10 size classes with parallel bins which can have different chemical compositions. Thus in total, the module tracks 20 size bins which cover diameters ranging from 3 nm to 10 μm and are divided to three subranges each with an optimised selection of processes and compounds.

P3624, L11-27: These two paragraphs simply describe comparison between simulated results and measurements. However the primary aim of this manuscript is to incorpo- rate the microphysical model into the GCM. Therefore the authors should write how to incorporate the SALSA module into the ECHAM model in half, and in the other half about comparisons with measurements.

We have added description of the implementation of SALSA to ECHAM in the revised manuscript.

P3626, L15-17: It is difficult to understand why it is a result of the previous sentence.

This has been rephrased:

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To obtain this, the aerosol size distribution is divided into 3 subranges, each with different bin widths and degree of aerosol external mixing.

Figure 1: It is difficult to understand section c (insoluble, cloud activation). Please describe more clearly in section 2.3. Does the section c means an insoluble particle with soluble material by internal mixing? If so, why doesn't the section 2c exist?

The figure has been changed to new version. In the new version this categorisation is removed as it only depicts the subregion 3

P3630, L1-7: Please describe "analytical predictor of condensation", "collision scheme", and "operator splitting technique", even if in appendix.

The techniques have been explained in detail in the references given. However, we have included short explanatory sentence in conjunction with each of the methods.

The mass transfer of gaseous H_2SO_4 onto particle surfaces is calculated using Analytical Predictor of Condensation (APC) scheme (Jacobson, 1997a) with the saturation vapor pressure set to zero. APC scheme solves the mass transfer without iteration conserves mass exactly, and is unconditionally stable. For the coagulation collision rate, we use the expression by Lehtinen and Kulmala (2003). The coagulation collision scheme is an accurate discrete method for calculating coagulation of nucleation mode particles. For simultaneous calculation of nucleation and condensation we use the operator splitting technique developed by Jacobson (2002). Operator splitting techniqur allows for realistic competition among size bins for sulphuric acid available for nucleation and condensation.

Additionally the citation for coagulation collision rate has been corrected to Lehtinen et al 2004 changed to Lehtinen and Kulmala 2003

Lehtinen, K. E. J. and Kulmala, M.: A model for particle formation and growth in the atmosphere with molecular resolution in size, Atmos. Chem. Phys., 3, 251-257,

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doi:10.5194/acp-3-251-2003, 2003

P3632, L25-26: "the growth of particles over the boundary between 2nd and 3rd sub- range has to be treated separately". I cannot understand it. What is treated separately? P3632, L26 - P3633, L1: Why isn't 2a4 transferred to 3a1? P3633, L4: Why isn't 2b4 transferred to 3b1?

This paragraph has been rephrased:

In SALSA, the compounds have mass tracers only in subranges 1 and 2, and therefore the growth of particles over the boundary between the 2nd and the 3rd subrange has to be treated separately. When particles grow over the boundary, all mass mixing ratios in 2a4 are transferred to 3b1. The particles from 2a are transferred to 3b to since both subranges contain aged particles. The corresponding particle number mixing ratio is calculated from the transferred mass using the fixed bin mean diameter of bin 3b1 Similarly, the mass from insoluble bin 2b4 is transferred to bin 3c1 in case the particles grow across the subrange boundary. The soluble mass fraction from 2b4 is transferred to water soluble fraction of 3c1.

P3636, L8-10: After all, which is the mode radius, 0.075 or 0.04, and the standard deviation, 1.59 or 1.8?

This sentence has been clarified.

Carbonaceous particulate emissions so that all carbonaceous mass is are emitted in subranges 1a, 2a or 2b assuming lognormal distributions by Stier et al. (2005) with a median particle radius $\bar{r} = 0.075$ and standard deviation $\sigma = 1.59$ (adapted from the distributions by Dentener et al. (2006) which have $\bar{r} = 0.04$ and $\sigma = 1.8$).

P3636, L24: Does "biomass burning" include vegetation fire and biofuel? If so, what is ratios of 1a and 2a to 2b for biogenic and fossil fuel?

SO_2 is in gaseous phase and as such it is not associated with particles. However, it participates particle nucleation and condensation after oxidation to SO_4.

P3636, L24: How is it different between wildfire and biomass burning? Be clear the differences among vegetation fire, biofuel , wild fire, and biomass burning.

Biomass burning is regarding burning of mainly wood in stoves for heat, while the vegetation fires are forest or grassland fires initiated by humans. Biomass burning has been changed to biofuel for consistency.

P3640, L8, L9: Is Liu et al. (2001) a right reference?

The referee is correct, the reference is erroneus, the right reference is Liu etal.(2005b). We have corrected the reference.

Figure 5: Describe why there are two or three size modes in M7 in section 4.2. Readers cannot understand it because there are any description on M7 in this manuscript. Also the authors have to confirm the observed size distributions appropriately measure nano particles between 1 to 10 nm shown in Fig. 5. Moreover, is it right in axis of the size? Why don't the authors compare and discuss the size distribution over 1 μm although this model treat from 3 nm to 10 μm?

We included the size distribution of particles under 1 um in diameter because the main emphasis of this figure was to compare how the models compare with observations and the available data for these stations includes only particles under 1um.

For the stations Pallas (Hatakka et al. 2003), Melpitz (Engler et al. 2007) and Hyytiälä (Suni et al. 2003), the data is available as shown in the Figure 5. For other stations the plot doesn't include data for particles below 10nm in diameter.

Engler, C., Rose, D., Wehner, B., Wiedensohler, A., Brüggemann, E. et al.: Size distributions of non-volatile particle residuals (Dp < 800 nm) at a rural site in Germany and relation to air mass origin. Atmos. Chem. Phys. 7: 5785-5802, doi:10.5194/acp-7-5785-2007, 2007

Hatakka J, Aalto T, Aaltonen V, Aurela M, Hakola H, Komppula M, Laurila T, Lihavainen H, Paatero J, Salminen K, Viisanen Y: Overview of the atmospheric research activities

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and results at Pallas GAW station, Boreal Environmental Research 8 (4) : 365-383 (Dec 2003)

Suni T, Rinne J, Reissell A, Altimir N, Keronen P, Rannik U, Dal-Maso M, Kulmala M, Vesala T: Long-term measurements of surface fluxes above a Scots pine forest in Hyytiala, southern Finland, 1996–2001, Boreal Environmental Research 8 (4) : 287-301 (Dec 2003)

Figure 8: As same as Fig. 5, why don't the authors compare and discuss the size distribution over 1 μm although this model treat from 3 nm to 10 μm?

The observations do not have any measurements between 1 and 10 um, therefore we chose to use plots without this size range. However, as sea-salt emissions between 1 μm 10 μm are important in the marine environment, we have included 1 to 10 um and added one sentence to text as follows:

For particles between 1 μm and 10 μm in diameter, SALSA shows slightly lower particle concentrations than M7.

P3652, L26-27: "SALSA shows much better agreement with observed concentrations than M7." I cannot agree it. I think from Fig. 8 that M7 is in much agreement with observations during 10 to 100 nm, which also have a peak in this size range. The authors have to evaluate objectively.

We have rephrased the part in question to indicate that in other parts SALSA is better than M7 and vice versa. Rephrasing is as follows:

Especially for particles 0.01 μm–0.1μm in diameter SALSA shows worse agreement with observations than M7, for the particles with diameters ranging 0.1 μm–1μm SALSA shows better agreement with the observations than M7.

P3652, L29 - P3653, L1: "Moreover, SALSA reproduces the magnitude of the observed concentrations quite accurately." Again I cannot agree it from Fig. 8.

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Referee is correct, based on these figures we cannot conclude this. We have removed this sentence.

P3653, L13: Is Martonchik et al. (1998) a appropriate reference? Probably Kahn et al. (2005) is more appropriate for MISR aerosol retrieval. Kahn, R., B. Gaitley, J. Martonchik, D. Diner, K. Crean, and B. Holben (2005), MISR global aerosol optical depth validation based on two years of coincident AERONET observations, J. Geophys. Res., 110, D10S04, doi:10.1029/2004JD004706.

Reference is added.

P3654, L10-11: Do you have any specific data and references that emission in the old emission inventory used in this study is lower than that in new ones?

Emissions in developing countries such as China and India have grown from 2000 to 2008, however, the growth globally is very small due to restrictions on emissions in Europe and USA. (Granier et al. 2011) The reference is added to revised manuscript.

Granier, C., Bessagnet, B. Bond, T. C., D'Angiola, A., Denier van der Gon, H., Frost, G. J., Heil, A., Kaiser, J. W., Kinne, S., Klimont, Z., Kloster, J., Lamarque, J. F., Liousse, C., Masui, T., Meleux, F., Mieville, A., Ohara, T., Raut, J. C., Riahi, K., Schultz, M., Smith, S. J., Thomson, A. M., van Aardenne, J., van der Werf, G. R., and Van Vuuren, D.: Evolution of anthropogenic and biomass burning emissions of air pollutants at global and regional scales during the 1980-2010 period, Climatic Change, 109(1-2):163-190. doi:10.1007/s10584-011-0154-1

P3654, L16-23: A few parts of the contents in this paragraph is same as in the previous paragraph. Integrate two paragraphs.

We have integrated the two paragraphs as follows:

Figure 9a shows the observed clear sky annual mean aerosol optical depth (AOD) composite of the satellite retrievals. Figure 9b shows the difference with SALSA to satellite retrieval, while Fig. 9c has the difference with M7. Although global annual

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mean aerosol optical depth simulated with SALSA is smaller than satellite retrieval the spatial distribution is quite good. The AODs over ocean gridpoints in the tropics are generally within 0.02 of the satellite retrieval (Fig. 9). With both models the high latitudes have much lower AOD than satellite retrievals, especially over Siberia in Russia, over Canada and in the coast of Alaska the AOD is underpredicted with 0.2 smaller values than in MISR retrieval. The AOD is lower probably partly due to old emission inventory (Granier et al. 2011) and partly due to too low transport (Bourgeois and Bey 2011) from tropics and mid-latitudes towards the poles. Furthermore, MODIS sees a band of higher AOD around Antarctica which the models do not reproduce. However, the differences in this area are partly caused by the cloud fraction affecting the satellite aerosol retrieval Shi et al. 2011). Furthermore, in the Saharan dust bloom over North Atlantic Ocean, M7 shows AOD 0.15 higher than observed while SALSA shows only 0.02 difference to the observed AOD (Fig. 9). In Europe and the east coast of USA, the AOD with SALSA is captured mainly within 0.02 of the observed while M7 shows differences over 0.05.

Figure 10: Why isn't this figure a simple scatter plot between AERONET and the models? It is difficult to evaluate the performance of the models by readers.

In our view the scatterplot had too many overlapping datapoints making it difficult to compare the model results. With box and whiskers plot it is easy to see relative amount of datapoints in a given range and more easily to compare the performance between the models.

Figure 11: Why isn't Angstrom exponent from MISR used as same as AOD?

Because no publications on the validation of Angstrom component of the MISR instrument was found, we feel that Ångström exponent from MISR is too unreliable, hence it is not used for land areas.

Technical corrections

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P3639, L26: Correct “assymmetry” to “asymmetry”.

Corrected as requested.

P3656, L25: Add comma after “black carbon”.

Added as suggested.

Table 7: Add horizontal lines before and after the line “Organic carbon”.

A line has been added.

Figure 3: Write unit in this figure (cm−3).

Unit has been added as suggested.

Interactive comment on Geosci. Model Dev. Discuss., 4, 3623, 2011.

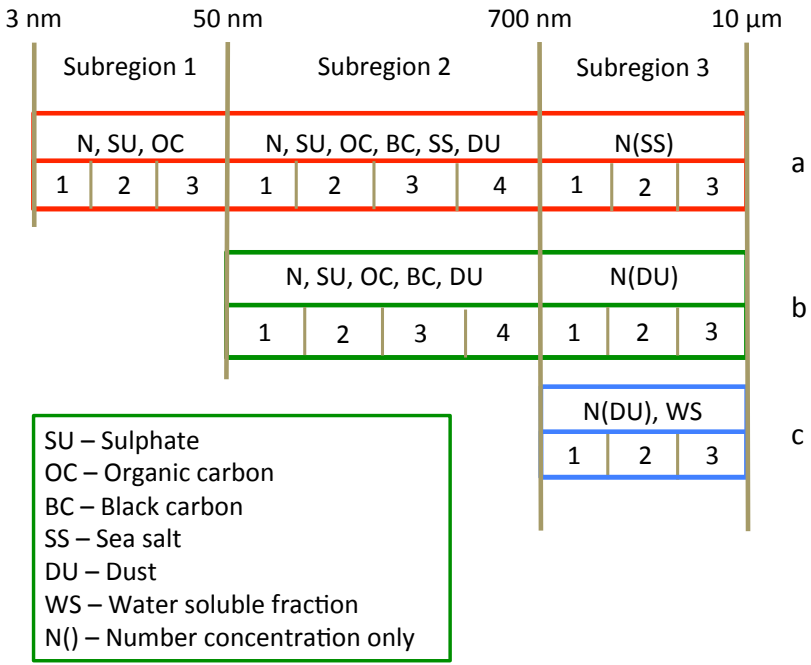

Fig. 1. Figure 1 of the manuscript

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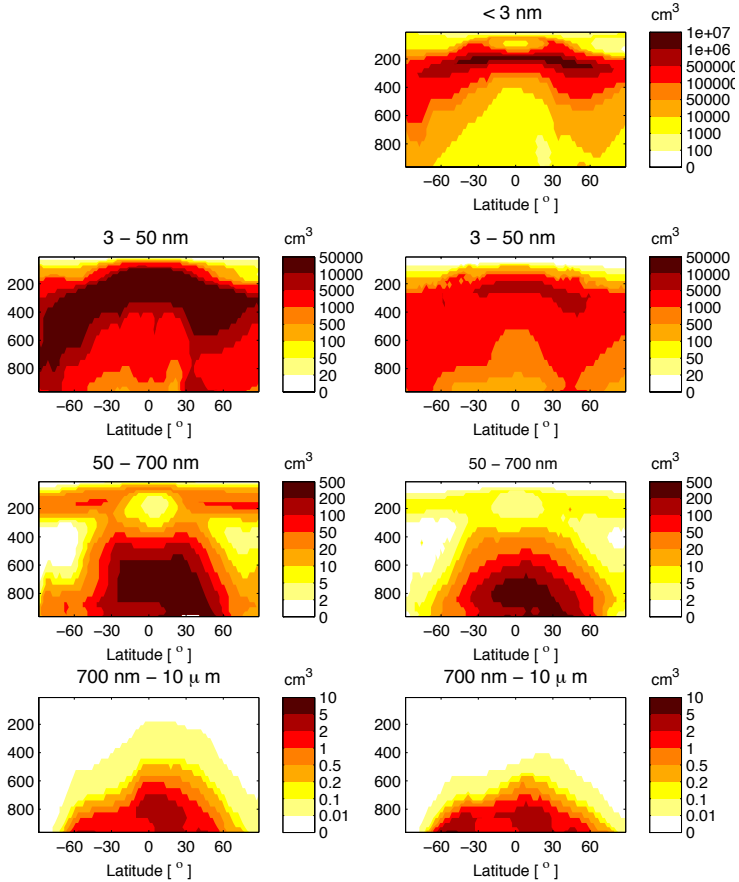

Fig. 2. figure 3 of the manuscript

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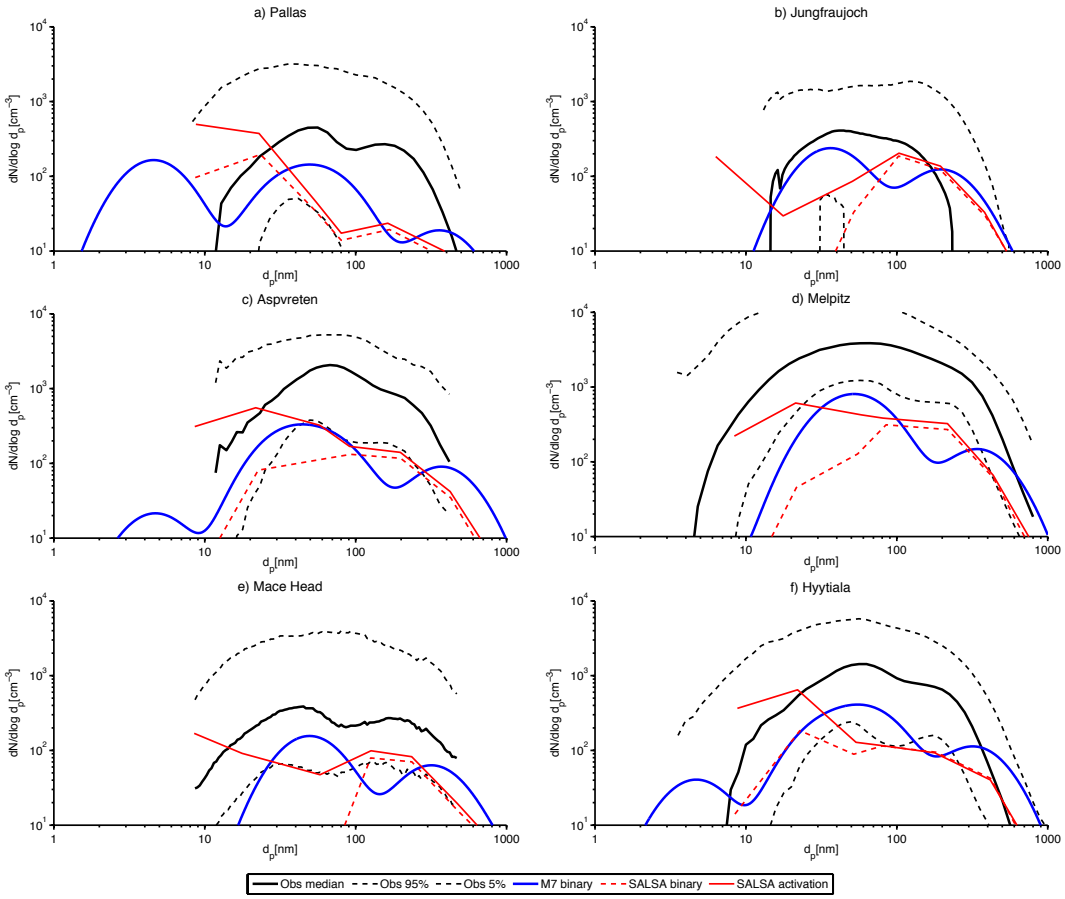

Fig. 3. Figure 5 of the manuscript

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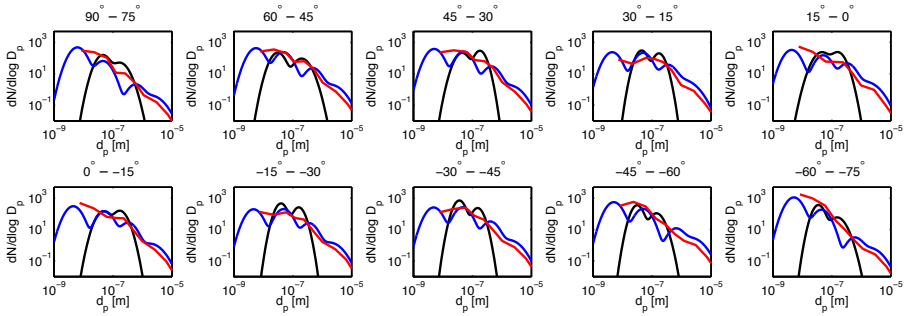

Fig. 4. figure 8 fo the manuscript

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