



## ***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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**Review of the paper ? Evaluation of the sectional aerosol microphysics module SALSA implementation in ECHAM5-HAM aerosol climate model ? by Bergman T et al. This paper presents the integration of the sectional aerosol model SALSA in ECHAM5-HAM climate model and its evaluation. English should be checked by a native English speaker. I corrected some mistakes, but there are too many. It is sometimes difficult to understand what the authors mean. The model presentation is confusing. For example, the separation between soluble and insoluble is not clear, as well as the partitioning of mass and number concentrations. Sea-**

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salt emissions are calculated by using a radius at 80% humidity. This should be corrected to correctly account for variations in relative humidity for soluble particles. For wet deposition, dry deposition and sedimentation, it is not clear which diameter is used for each section. For soluble particles, the water uptake should be considered when computing the section diameter to be used for deposition. Sedimentation is very low compared to M7. It should be checked that this is not due to the numerical sedimentation limitation with the time step. In the evaluation, the authors are not always objective in the comparison. For example, in Fig 5, they compare models at one specific particle diameter (200nm), whereas the comparison would give opposite results if a diameter of 500nm was chosen. In Figure 6, one model seems to be better in some cases, while the other model seems to be better in other cases. Computing statistics would be a fairer way to decide which model is better. The model is evaluated by comparisons to M7 and observations. For surface concentrations, it would be valuable to compute metrics which are commonly used to evaluate models (see Boylan and Russell, 2006). More detail on the meteorology should be given, and the length of the simulation should be given. I don't understand why there are large differences in meteorological fields depending on whether M7 or SALSA is used. I recommend the article to be published if the authors can address the specific comments below.

We thank you the reviewer for such a thorough work and huge contribution to improve our manuscript. Below we have addressed the reviewers specific comments. Bold face indicates a comment by the reviewer and below is our answer to specific question. Most of the above mentioned problems are contained in the specific comments so they are gone through here. In addition to specific comments, authors inform that the language and clarity has been corrected throughout the revised manuscript.

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

## Specific comments:

### Abstract

**C: - I8: What do you mean by “ 20 size sections with 10 size sections in size space “? What about the other 10 sections?**

We have rephrased the sentence:

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  $\mu\text{m}$  and are divided to three subranges each with an optimised selection of processes and compounds.

**C: - L13: I do not understand what “ modal “ refers to here. Maybe better to remove it from this sentence.**

The word is removed as suggested.

**C: - L14: replace “ sea salt mass “ by “ sea-salt mass “.**

Corrected as suggested.

**C: - L15: What does AOD stand for? Please detail at the first occurrence.**

Aerosol optical depth (AOD) is detailed as suggested.

**C: - L22: Is it acceptable to have concentrations within a factor five of the observations?**

We have rephrased this as follows:

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Surface concentrations of sulphate and carbonaceous species have an annual mean within a factor of two of the observations. The simulated sea salt concentrations reproduce the observations within a factor of two, apart from the Southern ocean over which the concentrations are within a factor of five.

**C: - L22 and L23 are in contradiction. You first say that sea salt is within a factor of five of the observations, and then you say that sea-salt observations are reproduced less accurately (than sea salt!)**

These sentences have been rephrased.

Surface concentrations of sulphate and carbonaceous species have an annual mean within a factor of two of the observations. The simulated sea salt concentrations reproduce the observations within a factor of two, apart from the Southern ocean over which the concentrations are within a factor of five.

**C: - L23: Replace “ sea salt concentrations “ by “ sea-salt concentrations”**

Replaced as suggested.

**C: - L27: Replace “ than the observed “ by “ than observed “.**

Replaced as suggested.

## Introduction

**C: - L27, p3625: For examples of other modal models, you may add these 2 citations: Binkowski FS; Roselle SJ, Models-3 community multiscale air quality (CMAQ) model aerosol component. 1. Model description, JOURNAL OF GEOPHYSICAL RESEARCH/ATMOSPHERES Volume: 108 Issue: D6 Article Number: 4183 DOI: 10.1029/2001JD001409 Published: MAR 26 2003 Sartelet KN; Hayami H; Albriet B; et al., Development and preliminary validation of a modal aerosol model for tropospheric chemistry: MAM. AEROSOL SCIENCE AND TECHNOLOGY Volume: 40 Issue: 2 Pages: 118-127 DOI: 10.1080/02786820500485948 Pub-**

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lished: FEB 2006

We have added the references as suggested.

**C: - L3, p3626: limitations of modal models related to the growth of particles are illustrated in Sartelet et al. (2006)**

We have added the citation.

**C: - L6, p3626: What do you mean by “ separate retrieval “?**

We have rephrased the sentence.

In this method, aerosol properties require an off-line calculation so this approach can be considered rather inconvenient when interpreting model simulations and comparing the simulations with atmospheric measurements.

**C: - L22-24, P3626: I do not agree. Sectional models do have a pre-defined assumption of the particle size-distribution shape: it is a section!**

Referee has a point and thus we have rephrased the sentence as follows:

The main advantage of SALSA compared to modal models is that the sectional method is more flexible in presenting the particle size distribution, which can significantly affect e.g. cloud activation predictions.

**C: - L9-L11, P3628: Please rephrase. I could only understand after looking at the figure, I think the sentence is not clear enough (as well as the legend of the figure).**

We have rephrased the sentence:

In the setup used in this study the size distribution of SALSA consists of 10 size sections with parallel chemical compositions (i.e. some degree of external mixing) and thus simulates 20 bins in total (see Fig. 1).

**GMDD**

4, C1735–C1760, 2012

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We have also modified the Figure 1, to show distribution of compounds in sections more clearly.

**C: - L11, P3628: For clarity, please replace “ external mixing “ by “ chemical composition “**

Replaced as suggested.

**C: - Figure 1. For clarity, it maybe better not to call “ a, b, c “ parallel sections but rather chemical composition. Then the legend of Figure 1 can be rewritten by something like ? for each size section, particles are separated depending on their chemical composition, that is whether they are soluble, insoluble or insoluble with a soluble coating enabling cloud activation ?. Also, from the figure, it seems that the possible chemical components of particles of one subrange are detailed below the subrange. Is this correct? For example, may soluble particles contain dust or black carbon and may insoluble particles contain water soluble fraction?**

Figure 1 has been corrected to clearly show the distribution of compounds within the differenc subranges.

**C: - Table 1: For sections 3b3 and 3c3, there may be typos: the max diameter are probably 10 rather than 1**

There was a typo in Table 1. Authors have corrected the maximum diameter as 10  $\mu\text{m}$ .

**C: - L15, P3628: What do you mean by “ external mixing of particles is limited to subranges 2 and 3 “? Is it that insoluble particles are not considered in the first subrange? If it is so, it is clearer to say it explicitly.**

Yes, subrange 1 includes only sulfate and organic carbon with only one size section per size range. We have removed this mention of external mixing. The lack of external mixing is mentioned in the paragraph explaining the subregion 1.

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**C: - Table 2: What is dry deposition not considered for 1a and 2a particles, if it is considered for others? The influence of dry deposition is minimum for particles of diameters about 1 micrometer, but the influence increases as the diameter increases and decreases.**

Table 2 was erroneous, the wet deposition is included also for 1a and 2a. Table has been corrected to reflect this.

**C: - Table 3: What do you mean by “ the number tracer “?**

We have changed this to number concentration. With just number concentration/mixing ratio we can also calculate the corresponding mass.

**C: - L20-21 and L22-23 are in contradiction. In the third subrange, number and mass are considered according to L21. Then it says that mass is considered only for particles below 700nm. Do you mean above 700nm? Considering mass as a prognostic variable is crucial for large particles as they account for most of the mass of particles.**

We have rephrased this paragraph as follows:

To reduce the computational burden of the module, the only the most relevant chemical compounds and microphysical processes are included for each size range. The simulated processes are listed in Table 2 and the compounds in Table 3. Note that in subrange 3c the chemical compounds are not simulated explicitly but lumped into insoluble (i.e. dust) and soluble components. The soluble component includes water soluble compounds (SU and OC) transferred from subrange 2b after growth over 700 nm.

**C: - L1, P3629: Can you detail which water soluble compounds you are referring to? Sulphate, sea salt are water soluble content, as can also be organic carbon.**

The water soluble compound is a mixture of sulphate and organic carbon that are transferred from subregion 2b. The paragraph is rephrased as follows:

To reduce the computational burden of the module, the only the most relevant chemical compounds and microphysical processes are included for each size range. The simulated processes are listed in Table 2 and the compounds in Table 3. Note that in subrange 3c the chemical compounds are not simulated explicitly but lumped into insoluble (i.e. dust) and soluble components. The soluble component includes water soluble compounds (SU and OC) transferred from subrange 2b after growth over 700 nm.

**C: - L6, P3629: Can't particles in subrange 1 coagulate with themselves? Is only coagulation with larger particles considered? Why?**

The coagulation of the particles in subrange 1 has been neglected to reduce the computational time. Coagulation of the particles in subrange 1 have very small effect on overall as the coagulation with larger particles is much more favourable. Discussion on this has been added as follows:

Neglecting self-coagulation may cause some error in the smallest size bins; however, generally coagulation with larger particles is much more likely than with equal sized particles.

**C: - L13, P3629: Does the soluble bins include dust? If so, why? Because dust is not soluble. Does the insoluble bins included sulfate? If so, why? Because sulfate is soluble. Black carbon is also insoluble but it also goes into the soluble bins?**

Model in itself allows this because buildup of soluble compounds on the particles would make them susceptible for cloud activation. But in this study the transfer from insoluble to soluble is not used, therefore there is no dust or black carbon in 2a. However, because this a model description paper we have described also this possibility for future users.

**C: - L16, P3629: For clarity, replace “ three parallel bins “ by “ three parallel**

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## chemical composition “.

Replaced as suggested.

### **C: - L18, P3629: What is soluble dust?! Is it dust mixed with water soluble compounds? What are these compounds? Why are they not considered for sub-range 2?**

3b consist of particles grown from 2a and as such may contain dust, black carbon, organic carbon, sea salt and sulphate. Therefore particles may have include soluble compounds. To clarify we have rephrased to indicate that it contains all compounds, although masses of distinct compounds is lost and it is treated as dust having density of dust or sea salt.

The three size sections in subrange 3 cover the particle size from 700 nm to 10  $\mu\text{m}$  and have three parallel chemical compositions. Most of the particles originate from natural sources. The three externally parallel compositions sea salt, aged particles from subrange 2 and insoluble dust with water soluble coating. The water soluble compounds sulphate and organic carbon grown from subrange 2b are treated as one compound (water soluble – WS) within the insoluble dust group.

### **C: - L20-21, P3629: in contradiction with L25-26 P3628.**

We have rephrased so that it indicates that number mass concentrations of all parallel sections and mass concentration of water soluble material in 3c are prognostic variables. This paragraph is rephrased as follows:

The three size sections in subrange 3 cover the particle size from 700 nm to 10  $\mu\text{m}$  and have three parallel chemical compositions. Most of the particles originate from natural sources. The three externally mixed parallel compositions sea salt, aged particles from subrange 2 and insoluble dust with water soluble coating. The water soluble compounds sulphate and organic carbon grown from subrange 2b are treated as one compound (water soluble – WS) within the insoluble dust group.

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**C: - L6-7, L11, P3629. For clarity, you should consider moving all the processes description to section 2.4.**

L6-7 and L11 are removed from this section and moved to section 2.4.

**C: - L25-27, P3629: Why is intra-sectional coagulation omitted? Especially for nanoparticles, intrasectional coagulation will make the section diameter grows, and it could be an important process. This should be discussed in the manuscript.**

We have added the following discussion to this paragraph:

Neglecting self-coagulation may cause some error in the smallest size bins; however, generally coagulation with larger particles is much more likely than with equal sized particles.

**C: - L10, P3630: What organic compounds do you consider? Do they partition between the gas and the particulate phases?**

The model includes organic vapors as an optional component and in this study there is no organic vapors. However, authors present the possibility to use organic vapors in the model.

**C: - L19-21, P3630: Why us binary nucleation parameterisation not applied everywhere? How are the cases defined?**

The sentence is rephrased to reflect that the binary nucleation is calculated throughout the atmosphere. However, we have implemented the activation and kinetic nucleation parameterisation for the boundary layer as a choice, and in this study we looked at the affect of activation type nucleation on the number size distribution of particles on the surface (section 4.2). Rephrasing is as follows:

For the calculation of nucleation the current setup uses the parameterised sulphuric acid-water binary homogeneous nucleation parameterisation (Vehkamäki et al., 2002)

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in the free troposphere, and three optional mechanisms in the boundary layer: binary homogeneous nucleation, and two empirical parameterisations for kinetic (Sihto et al., 2006; Riipinen et al., 2007) and activation nucleation (Kulmala et al., 2006; Riipinen et al., 2007).

**C: - L7-15, P3631 and L23-24, P3631: This part can be shortened. You don't need to detail the Lehtinen et al and the Antilla et al parameterisations here, as they are not used. You don't need to detail the subject of future studies, that's to be done in the conclusion.**

We have corrected as suggested.

**C: - L14-15, P3632: Is it possible to have cloud droplets and produce sulphate in-cloud without preexisting particles? How are the cloud droplets formed?**

Current version does not calculate the aerosol activation to cloud droplets explicitly. Instead the cloud parameterisation calculates cloud droplet concentration using the Tompkins cloud scheme. Sulphate is calculated according to the scheme by Feichter et al., 1996. It is assumed that soluble particles larger than 50 nm form cloud droplets.

**C: - Paragraph 2.4.2: Why are heterogeneous reactions at the surface of particles not modeled? Some parameterisations such as the one of Jacob (2000). Jacob DJ, Heterogeneous chemistry and tropospheric ozone, ATMOSPHERIC ENVIRONMENT Volume: 34 Issue: 12-14 Pages: 2131-2159 DOI: 10.1016/S1352-2310(99)00462-8 Published: 2000**

This is the first implementation of the sectional model and the detailed heterogeneous reactions at the surface of the particles are beyond the scope of the study. We will include the effect of heterogeneous reactions in our model in the future.

**C: - L17-23, P3632: What is done in this study then to transfer insoluble sections to soluble sections?**

In this study the transfer from insoluble sections to soluble is not used. As this a model

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description paper we have detailed all implemented options although all of them are not scrutinized within this study.

**C: - Table 1: Add a column with the mean diameter, as it is often used to transfer from number to mass**

The column with volume mean diameter has been added to Table 1.

**C: - L26, P3632 and L27, P3633: Why is 2a4 not transferred to 3a1 rather than 3b1 (soluble section should be transferred to soluble rather than insoluble section?). Why do you need to compute the number mixing ratio from the mass mixing ratio? Is it that the subranges 1 and 2 only have mass mixing ratio and the subrange 3 only number mixing ratio?**

This paragraph has been rephrased as follows:

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.

**C: - L4, P3633: Why is 2b4 not transferred to 3c1?**

The 2b4 is indeed transferred to 3c1, see the previous comment.

**C: - Table 4: What do you mean by “ has been defined separately “. How were they defined?**

The sentence has been left out. Sentence was meant to point out that it is different

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from the other sections and similar with M7.

**C: - L5, P3635: For what kind of calculation do you refer to Kerkweg? Is it for more detailed model presentation?**

We have rephrased the sentence:

A more detailed description of wet deposition can be found in Kerkweg et al.

**C: - L12-15, P3635: For soluble mode, do you consider the water uptake of particles for computing the bin mean radius?**

The water uptake for soluble bins is taken into account in the calculation of bin mean radius used when calculating sedimentation. The latter part of this paragraph has been rephrased starting after the formula (line 11):

where  $R_p$  is the particle radius,  $\mu$  is air viscosity,  $u_\infty$  is wind velocity and  $C_c$  is the Cunningham slip correction factor. The particle radius is assumed equal to the sectional mean radius after the water uptake.

**C: - L17-18, P3635: Rather than limiting the sedimentation velocity, why do you not limit the time step and subcycle the temporal resolution for sedimentation? If you keep the time step constant and limit the sedimentation velocity, wouldn't you sometimes under-estimate sedimentation?**

The sedimentation uses the same formulation as M7. Furthermore, we have inspected the sedimentation velocities and they are well below the criterion.

**C: - L20, P3635: It has been rewritten from what?**

The seasalt emission routine has been modified from that of M7. The sentence has been rephrased:

The sea salt emission scheme has been modified compared to the M7 and therefore we provide a more detailed description of these emissions.

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**C: - Paragraph 2.6.1: Is there a reference which describes in more details the different carbon emissions?**

The emissions are detailed in Dentener et al.2006. The use of the AeroCom emissions for year 2000 (Dentener et al. 2006) is explained in the section 2.6.

**C: - L7-8, P3637: How are primary emissions of SO<sub>4</sub> divided between the sub-ranges 1a, 2a and 3b?**

The mass size distribution of  $SO_4$  is calculated with the same lognormal distribution that is used in M7. Contribution to each bin in each subrange is calculated from this size distribution. This paragraph is rephrased as follows:

Most of the anthropogenic sulphur – 97.5% – is emitted as  $SO_2$  and 2.5% is emitted as particulate matter  $SO_4$  (Dentener et al., 2006) In the standard version of SALSA, the primary particles are emitted to subranges 1a, 2a and 3b following the modal structure published by (Dentener et al., 2006) However, to facilitate the comparison to M7, the primary emissions are in this study described using the M7 modal parameters Stier et al. (2005)

**C: - P3638: All these parameterisations use a radius at 80% relative humidity (r<sub>80</sub>), which is probably different from the mean radius of a SALSA section. I assume that the mean radius of a SALSA section corresponds to the dry radius (r<sub>d</sub>) of particles. Then, for soluble particles, to calculate sea-salt emission, you need to use  $dF/dr_d$  rather than  $dF/dr_{80}$ . To do so, you can approximate  $dF/dr_d = dF/dr_{80} * dr_{80}/dr_d$ .  $dr_{80}/dr_d$  can be approximated using empirical relationships, such as those of Hanel. Hanel, G.: The properties of atmospheric aerosol particles as functions of the Relative humidity at thermodynamic equilibrium with the surrounding moist air, *Adv. Geophys.*, 19, 73?188, 1976.**

We have used factor indicated in Guelle et al. 2001. The relation there was  $r_d \approx 0.5r_{80}$ . We have included a sentence explaining how it is calculated:

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In the following formulae,  $r$  stands for radius at RH 80 %, dry particle mass flux is calculated with  $r_{dry} = 0.5r_{80}$ .

Guelle, W., Schulz, M., Balkanski, Y., and Dentener, F.: Influence of the source formulation on modeling the atmospheric global distribution of sea salt aerosol, *J. Geophys. Res.*, 106, 27509–27524, <http://dx.doi.org/10.1029/2001JD900249>, 2001.

### **C: - L25-26, P3640: why? Is aqueous chemistry not considered in both SALSA and M7?**

Both models consider aqueous chemistry using the same method. Nevertheless, the uptake of SO<sub>2</sub> is very much pH dependent and the differences in particle composition (resulting from differences in microphysical processing and different representation of size distribution) will have significant implications on liquid phase SO<sub>2</sub> chemistry. Also, most of the SO<sub>2</sub> is found in the lower atmosphere where the cloud patterns are not the same for SALSA and M7. With higher cloud cover lower atmosphere M7 produces more in-cloud sulfate.

### **C: - P3641: For wet deposition, dry deposition and sedimentation, which diameter is used for each section? Does the diameter include the water uptake for soluble particles?**

Water uptake is taken into account for sedimentation and dry deposition, while wet deposition is parameterised only by their dry size dependent coefficient  $R_i$ , mixing ratios of aerosol tracers  $C_i$  and total cloud water  $C_{wat}$  and cloud fraction for liquid and water clouds, ( $f^{liq}$  and  $f^{ice}$ ), respectively.

P3635 L3: Word wet has been added to indicate wet radius of the particle in case of dry deposition:

The aerosol deposition is calculated on-line using the bin number and mass to calculate the aerosol deposition velocity as a function of particle wet radius, density, turbulence

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and surface cover as in Stier et al. (2005).

P3635 L11: Word wet has been added to indicate wet radius of the particle in case sedimentation: where  $R_p$  is the particle wet radius,...

**C: - P3641: sedimentation is very low compared to M7. This may be due to numerical sedimentation limitation with the time step?**

Sedimentation speed is well below this limit at maximum  $v=0.04\text{m/s}$  while CFL criterion would impose a limit of  $0.15\text{ m/s}$  near the surface where the layer is the thinnest. Mostly the smaller sedimentation is caused by the lack of mass information of the black carbon particles larger than  $700\text{ nm}$  in diameter.

**C: - L 19, P3641: Why would the cloud cover be different between the two model runs? I thought that M7 and SALSA are based on the same meteorology? C: - L1-3, P3642: Why are there windspeed differences when using SALSA rather than M7?**

The nudging method constrains the large-scale meteorology to a similar state between the models. However, the nudging method permits local-scale meteorological processes, such as clouds, to respond to perturbations, method is described in Jeuken et al. (1996). With changing the aerosol description we will induce perturbations to local scale and in this case the result can be seen in e.g. the cloud cover and surface wind speeds.

**C: - L11-12, P3642: OC mass is in particles below 700microm in the model SALSA used. That may not always be the case in reality. Therefore, the term “this suggests that” is inappropriate.**

Referee has a point here and thus the suggestion has been rephrased to indicate similarity between models:

As the organic carbon mass in SALSA is associated only with subranges 1 and 2, the close agreement between the models suggests that most of the OC is in particles



below 700  $\mu\text{m}$  in diameter also in M7.

**C: - L18-19, P3643: “As dry . both models.” This sentence is not finished.**

The sentence is rephrased:

Contributions of dry and wet depositions are of similar magnitude (within 9% and 22% respectively) in both models.

**C: - Section 3.1.4: Why is sedimentation large for sea-salt, while it is low for sulfate and BC? Please explain why sedimentation and deposition are important sinks for sea salt but comparatively they are not for black and organic carbon.**

The individual mass concentrations of OC, SO<sub>4</sub> and BC are not known for subregion 3, while for sea salt the mass is calculated using sectional mean diameter for subregion 3.

**C: - L23, P3644: Why would the coagulation of particles be more important for black carbon than for other chemical compounds?**

The absolute difference between sources and sinks is actually smaller than for sulphate and organic carbon. However, for black carbon the difference constitutes large fraction of the total mass. The growth losses are implied by the model as difference between sources and sinks is transferred to subregion 3.

**C: - L24, P3644: Not clear: lower burden and lower removal than what?**

Lower than compared to other models. We have rephrased the sentence.

Lower burden and removal by dry and wet deposition and sedimentation than in the other models could indicate that a large fraction of the material is removed by the processes affecting particles grown over the 700 nm boundary in the aerosol model.

**C: - L3, P3645: Processes such as sedimentation depends on diameter rather than chemical composition. Why is it more important for sea salt than for black**

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## carbon?

The sedimentation is larger for sea salt than the black carbon because the mass black carbon associated with particles with diameters over 700 nm.

**C: - L22-24, P3646: What do you mean: most of H<sub>2</sub>SO<sub>4</sub> is used for nucleation in M7 and none is left for condensation? But there is enough H<sub>2</sub>SO<sub>4</sub> for nucleation and the parameterized condensation in SALSA? Is it because of process splitting in M7?**

In SALSA the condensation and nucleation is calculated simultaneously by method presented in Kokkola et al.2008 while in our current version of M7 these microphysical processes are calculated by process splitting.

**C: - Paragraph 4.1.: Boylan and Russel (2006) propose model goal and performance to evaluate how models simulate PM mass. Please compute the statistics described in their paper and include them in a table. Boylan James W.; Russell Armistead G., PM and light extinction model performance metrics, goal, and criteria for three-dimensional air quality models. ATMOSPHERIC ENVIRONMENT Volume: 40 Issue: 26 Pages: 4946-4959 DOI: 10.1016/j.atmosenv.2005.09.087 Published: AUG 2006**

We have added a table with Mean fractional bias (MFB) with following description:

The mean fractional bias (MFB) (Boylan and Russel, 2006) showing the deviation of the modeled concentrations from the observations are shown in Table 8. MFB indicates that SALSA performance with is lower than M7 for organic carbon, while for other species performance is slightly better.

**C: - L25-26, P3648: I don't understand how Table 7 gives information on the mass included in particles of diameter over 700nm.**

The mass is calculated using the fixed mean diameter of a section and the density of corresponding compound (sea salt or dust). And for water soluble fraction mass tracer

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is available. In the beginning of 3.1.4 there is the following paragraph indicating how it is calculated:

A large portion of the mass of sea salt and mineral dust is in particles larger than 700 nm in diameter. The mass of particles in this size range is estimated using the mean diameter of particles and their densities.

**C: - L12, P3649: What do you mean by “ limited transport “?**

This has been rephrased as follows: transport to continental sites is described inadequately in the model.

**C: - L18, P3640: Why looking at a single diameter of 200nm? If you look at a diameter of 500nm, the opposite to what is said here is observed. If one model performs better for a narrow diameter range than another, but if the opposite is observed for another diameter range, the conclusion should be that both models have difficulties. Changing the model configuration would probably change the narrow diameter range in which one model performs well.**

We have rephrased the sentence to correspond to a size range.

In Fig. 5 panels c, d and e we see that the concentrations of particles 100 *nm* to 300 *nm* in diameter are clearly higher with SALSA than with M7.

**C: - L26-28, P3650: Not true for Figure 5f and Figure5a, for which M7 is clearly better than SALSA. It seems that there is a shift between SALSA and M7 in the peak of the distribution corresponding to the largest diameters.**

Sentence has been rephrased accordingly.

The cloud activation occurs mainly in diameter range 50 *nm*–200 *nm* and compared to M7, SALSA shows better agreement to observations for these particles in polluted regions and worse agreement in regions with clean air

**C: - L8-10, P3651: How do you differentiate marine and polluted air from the**

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## graph?

In Mace Head the two different peaks represent different trajectories, one with lower concentrations from the Ocean and one with higher concentrations from polluted areas (Pers.comm. A. Asmi).

**C: - I don't agree from the results presented here that SALSA performs better than M7 for cloud activation studies. To have a fair comparison, you should compute different statistics of comparison to observations, and take a larger range of diameter: 100-700nm for example.**

We have rephrased the sentence:

While both models underestimate the number concentration in the size range relevant for cloud activation, in some cases SALSA performs slightly better than M7 and vice versa.

**C: - L25-26, P3651: How long are the simulations performed for? Do they represent a typical year? More detail on the meteorology should be given. Why don't you use the Heintzenberg data for one specific year that you simulate?**

We have added the simulation details in Section 2.2.:

The simulations with both models are performed for the period from July 2008 to December 2008. The spin-up spans the first six months and the analysis is done for year 2008.

The results reported by Heintzenberg are not from one single year but a collection of several measurement campaigns performed between 1966 and 1998. As far as we know, currently there are no other studies with such a wide range of measurements over the Oceans.

**C: - L20-23, P3652: From Figure 7, I do not agree that SALSA is better than M7. To look at the data objectively, you could see which of the two models give lower**

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## root mean square error.

We have calculated the RMS for both models and added following sentences.

The M7 has lower root mean square error of average number concentrations of 184.0 while SALSA has 225.1. With similar and quite large errors both M7 and SALSA perform quite equally well.

**C: - L26-27: No, between 0.01 micrometers and 0.1 micrometers, it seems that M7 is rather better.**

Sentence has been rephrase to indicate that models are good at different size ranges: Especially for particles 0.01  $\mu\text{m}$ –0.1 $\mu\text{m}$  in diameter SALSA shows worse agreement with observations than M7, for the particles with diameters ranging 0.1  $\mu\text{m}$ –1 $\mu\text{m}$  SALSA shows better agreement with the observations than M7.

**C: - L1, P3653: To which graph (latitude band) are you referring to?**

We are not referring to any single latitude band but instead an overall performance. In all of the size distributions the SALSA performs better or equally well for the second peak.

**C: - L1-7, P3653: What is the point of this paragraph? You already mentioned that SALSA has high low-diameter particle concentration.**

We have combined this information to the previous paragraph.

**C: - L7-9, P3654: If we look at the scale of Figure 9, it seems that both models AOD are within 0.02 of the satellite retrieval. Is it correct?**

No, the scale is between -0.2 and 0.2 and only white area is within 0.02.

**C: - L18, P3654. ? M7 has AOD at least 0.05 higher than SALSA ?. Why is this information interesting?**

We have rephrased the sentence.

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**C: - L1-3, P3655: This does not show from Figure 9. At high latitudes, M7 seems to perform better than SALSA. Also, SALSA seems to strongly underestimate AOD in the North half part of Africa, India and South East Asia.**

No, high latitudes are basically the same for both models. And mid-latitudes are incorrectly stated, this has been rephrased to indicate tropical Oceans. As for the Africa

- Please modify the conclusion following the previous comments.

#### **Minor comments:**

Other minor comments besides those mentioned below have been corrected as suggested, unless the wording has been rephrased.

- L22, P3638: Why is there an upper limit of 32 m s<sup>-1</sup> set for the wind speed?

Parameterisation has a limit of 32 m s<sup>-1</sup>

- Please be consistent in the notations used in the paper. For clarity, it is better to always use ? section ? rather to switch between ? section ? and ? bin ?.

- Figure 5: the diameters should have the same units in the Figure and in the explanations.

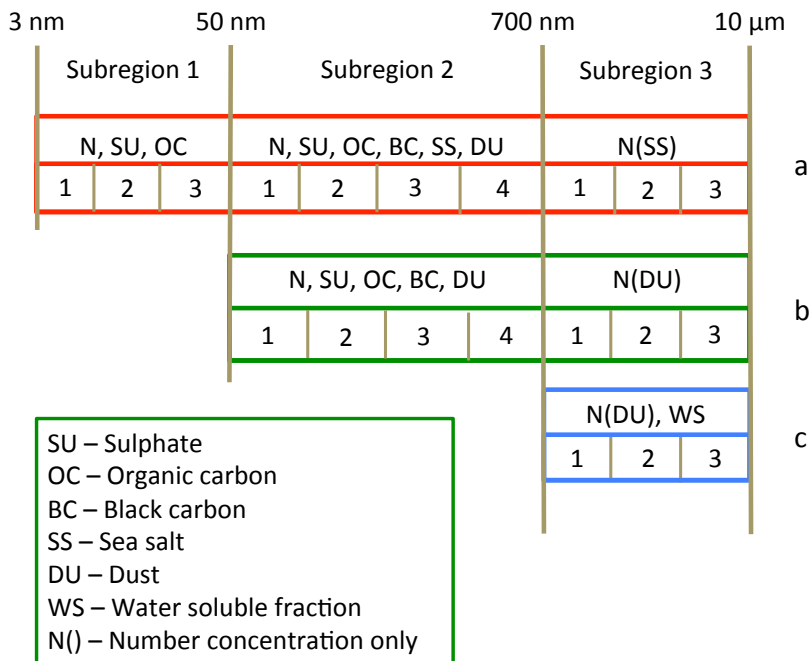
The Figure 5 diameters have been changed to nanometers.

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

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**Fig. 1.** Figure 1 of the manuscript

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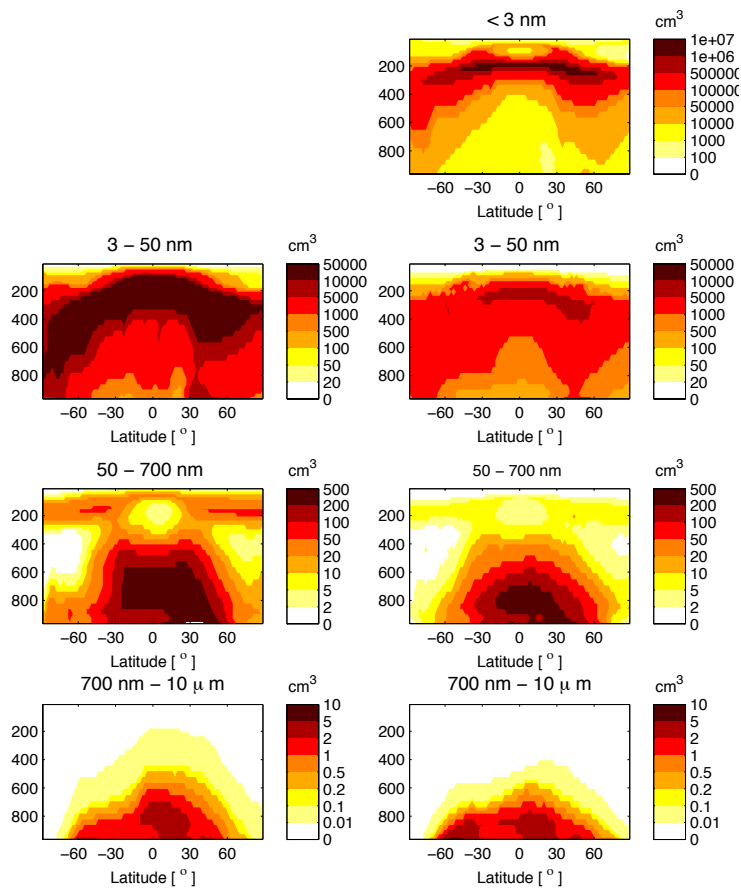


Fig. 2. Figure 3 of the manuscript



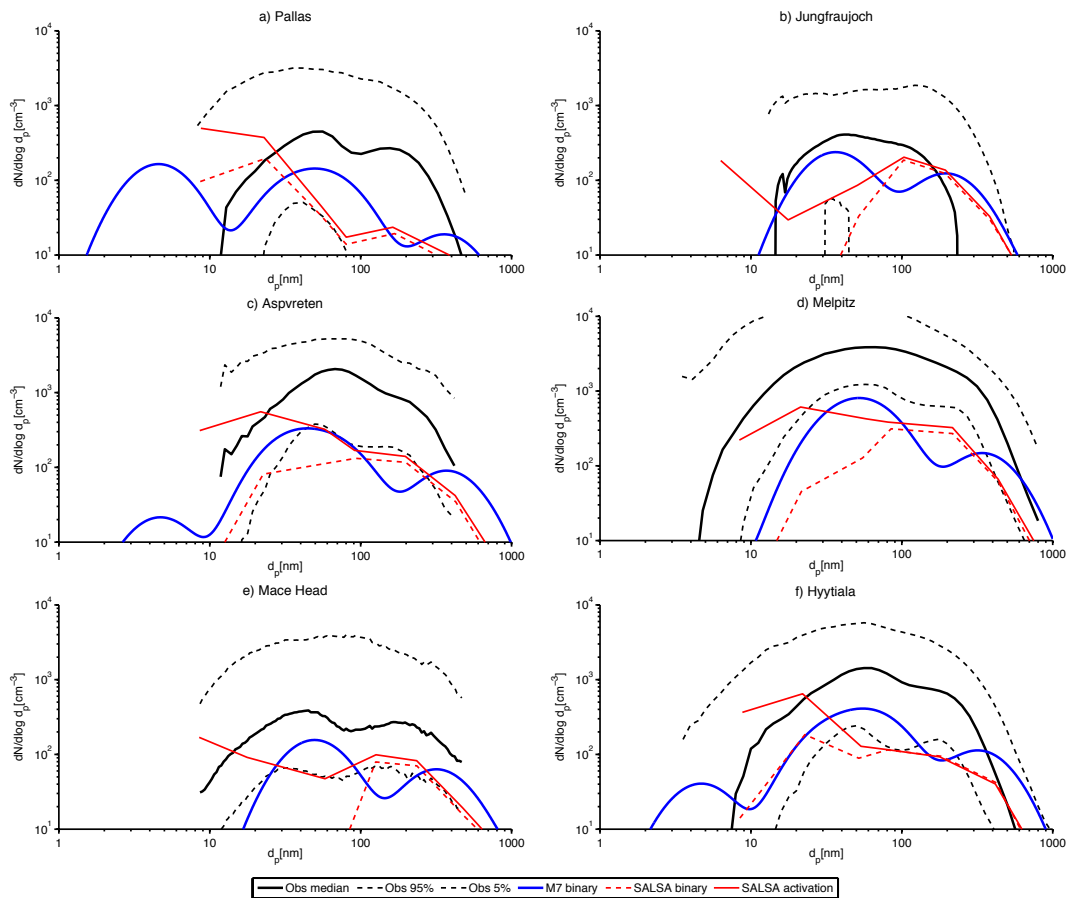


Fig. 3. Figure 5 of the manuscript

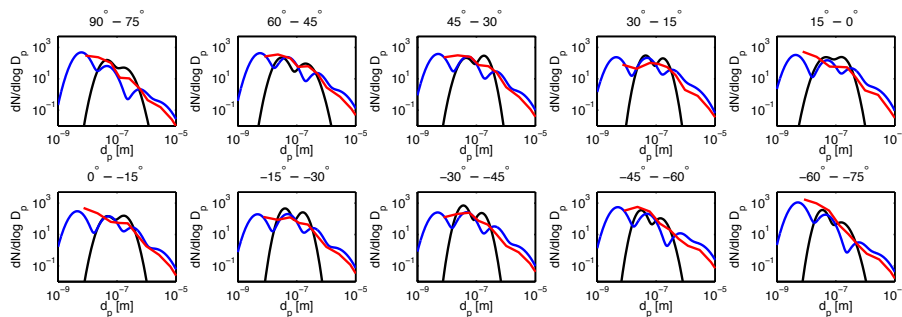


Fig. 4. Figure 8 for the manuscript

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