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Atmospheric Chemistry and Physics Discussions

Interactive comment on "SALSA – a Sectional Aerosol module for Large Scale Applications" *by* H. Kokkola et al.

Anonymous Referee #1

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1 General Remarks

In their manuscript H. Kokkola et al. present a new sectional microphysical aerosol module designed for the application in large scale atmospheric models. To minimize the computational cost, allowing for climate applications, they are aiming to reduce the number of prognostic parameters and processes through: a reduction of the number of size bins, the representation of selected components in different size ranges and the reduction of the system to the relevant processes for each size class. The approach is novel and could guide the development of a new generation of accurate, yet computationally affordable, aerosol models for climate applications.

Improved representation of aerosols and aerosol microphysics in global climate models are crucial for an improved understanding of anthropogenic climate change. The manuscript covers therefore a highly relevant research topic well in the scope of ACP. The manuscript is generally well written and the approach is innovative.

However, the manuscript has a few issues that should be addressed before publication. In particular the somewhat subjective (or overly CCN focused) selection of the relevant size-classes, the contained components and the simulated processes. I agree with reviewer 2 that this issue needs some clarification, although I would not go as far as making a full set of global simulations in a fully different model configuration (a big scientific study on its own) a requirement and will make some alternative suggestions below.

2 Major Issues

As mentioned above, the justification for the reduction of size-classes, the contained component and the simulated processes in this study is somewhat limited. The authors focus entirely on one objective measure: optimizing the selection of the limits between the size-bins through minimizing the error of predicted CCN as compared to a high resolution version of the box model with fixed aerosol content - assuming particles consisting purely of sulfate. The selection of the components in each of their size ranges is very subjective and needs to be backed by some facts. Also, one could argue that the optimization of the simulation of aerosol radiative properties might not be of equal importance. What effect would this have on the resulting model setup?

For example, the model setup assumes that black carbon is not present in any particles smaller than 0.05 micron and larger than 0.7 micron. This is a strong assumption, given that near source black carbon emissions contain a significant fine fraction and that mixtures of black carbon and dust have been observed in transition regime between

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dust and biomass burning emission. I would suggest to justify the selection of the representation in the components either by a explicit simulation with all components in all size sections, an interesting though tedious option, or alternatively through a representative literature review of size-resolved composition measurements.

Similarly, the authors argue that *"For sparse size bins, the moving center scheme has proven to be an efficient way to describe the time dependent development of particle size distribution (Jacobson, 2005) and this method is also used in the current aerosol model for particles in the subranges 1 and 2. Since aerosol dynamics do not affect particles larger than 730nm significantly, size bins in subrange 3 are treated in a fixed sectional grid to decrease the calculation time and the number of tracers in the model."*. It is true that aerosol dynamics do not affect large particles significantly in their static box model framework. However, differential sinks in a global model framework, that will certainly vary within the wide size range of the coarse bins in subregion 3, would in this case replace the effect of aerosol dynamics. Therefore, the computational benefit of this assumption needs to be balanced with the loss in accuracy - that requires some analysis.

In general, Discussion and Conclusions are too much based on your "opinion", that you refer to several times, than on facts and the results of this specific study. Please use more facts and less soft terms.

3 Specific Issues

- The flow and some English expressions could be improved throughout the papers.
- **page 17706, line 25**

  I suggest to add Boucher and Anderson (1995) to the generally somewhat brief

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list of references in the introduction.

- **Introduction, page 17708, line 2...**

  "another way of tackling this problem is..." - It might be worth mentioning that his approach is typically not consistent as the sectional, modal and bulk aerosol components are completely non-interacting in this case.

- **Introduction, page 17709, line 8**

  "... organic compounds, which have, until recently, been usually neglected..." - this is not really true, given that Jacobson's model treats organics since probably a decade...

- **Model description, page 17710, line 3**

  "... the degree of internal mixing usually decreases with increasing particle size..." - is this really generally true? I would assume this might not apply to the transition from (near source) Aitken to remote accumulation mode size ranges. Please back up by references.

- **Model description, page 17710**

  I think it will be important to test if the optimization procedure yields the same results under the assumption of realistic aerosol mixtures.

- **Model description, page 17711, line 7**

  Please add size range of this subrange. Also, I think it would be important to add a table with the actual size ranges for each section to allow for reproducibility.

- **Model description, page 17715, equation 12**

  Equation 12 does not seem to make sense for me. Please check for typographical errors.

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- **Model description, section 2.5**

  The description of the pre-calculation of the coagulation coefficients is very unclear. Explain in detail how this is done, how often, for what pressure levels, with what resolution. Are the model pressure levels assumed constant for the whole simulation period? If so, please provide an estimate of the introduced errors, at least back of the envelope.

  Also, the description of the interaction of the size sections is hard to understand. I am not entirely sure if particles in subrange 2b coagulate with subrange 1 (I assume yes, but the formulation is vague). Please clarify this section. If they do not coagulate, justify.

- **Model description, general**

  I think the split between model components that are included in the microphysical scheme (aerosol activation, cloud chemistry) and that are explicitly excluded (aerosol radiative properties) is not best suited for a modular set-up. Ideally, aerosol activation should be closely tied with cloud processing and wet deposition and the cloud chemistry with the general chemistry scheme, thus might be better dealt with elsewhere. On the other hand, aerosol radiative properties have to be calculated very specifically for this sectional setup and should be closely coupled to SALSA.

- **Model evaluation, section 3.1**

  I think it would be interesting to actually see the results of the tests with the timesteps, in particular for the full set of processes (coagulation, condensation, nucleation).

- **Model evaluation, page 17720, section 3.3**

  The discussion of insoluble vs. soluble is somewhat weak. You speak of the "right moment" of when the particles need to be transfered. However, what are

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you physically talking about. There are some interesting new results on coating thickness (e.g. Schwarz et al., JGR, in press - available from JGR webpage) that could be included in a more comprehensive discussion.

Also, as in the model optimization procedure the focus is again entirely on CCN prediction. However, the mixing state is also in particular relevant for the aerosol radiative properties in particular for absorption (e.g. Jacobson, 2001) but also - importantly for their sinks, both affecting the actual radiative effects (Stier et al., 2006).

Similarly, the presented tests of mixing on CCN are presented from a static, box model, point of view and might not apply to the real world, where enhanced hygroscopicity will certainly affect the sinks and the general availability of CCN. Therefore, your conclusion of "good confidence" might be somewhat strong, given the tremendous uncertainties.

- **Model evaluation, page 17721, figure 7**

  The dependence of the error in the SALSA setup shows a very different (and strange) dependency of the volume fraction than in the high resolution setup. Why?

- **Model evaluation, page 17722, figure 7**

  I would suggest to use "high-resolution" instead of "accurate" for the reference simulation, as it is not evaluated independently.

- **3.5 Technical details**

  I am confused with the references:

  The GCM ECHAM5: Roeckner et al. (2003)

  HAM, the aerosol module of ECHAM5: Stier et al. (2005)

  M7, the modal microphysical core of HAM: Vignati et al. (2004)

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So I assume you ran SALSA in ECHAM5-HAM with the processes adjusted for the sectional representation?

- **Discussion, page 17723, line 1**

  "for addition climate modelling" does not make sense to me.

- **Discussion, page 17723, line 3**

  You state that sectional models do not assume a functional form for the size distribution. This is true for high resolution sectional models. For low resolution models the functional form is clearly given by a superposition of monodisperse bins.

- **Discussion, page 17723, line 16**

  Unclear grammar.

- **Discussion, page 17723, line 3**

  You say that a radical reduction in the number of sections will degrade the results. What is radical? Add facts and justifications.

- **Discussion**

  Again, I would argue that aerosol radiative properties should be given equal consideration as CCN abilities in the discussion of mixing.

- **Discussion, page 17724, line 4**

  You argue that the simplification to one organic component "is not a major limiting factor of the model". This is certainly up for discussion. I fully agree that this is a reasonable representation for now, however, if it is a limiting factor is far from being known.

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- **Discussion, page 17723, line 10**

  Again, the references seem to be wrong, see above.

- **Conclusions, page 17724, line 21**

  Both dust and sea salt have important climatological effects. You probably want to refer to anthropogenic climate change here.

- **Conclusions, page 17723, line 3**

  You argue generally that "this fairly coarse sectional representation tracks the evolution of the particle size distribution much more accurately than a multimodal representation". Again, it would be good to stay to the facts and be clear that this applies to specific test cases and one specific modal aerosol module. It will be very interesting to see how general this statement is - once the model has been tested under the wide rage of realistic atmospheric conditions in a global model.

- **Conclusions, page 17723, line 12**

  The last sentence is very general, not based on results of the study (and seems to make limited sense as you mix mass and size-distribution). The implementation of SALSA in ECHAM5-HAM with a model setup in an otherwise almost identical model will certainly help to find the answer to this interesting question.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 17705, 2007.