Atmos. Chem. Phys. Discuss., 7, S9507–S9511, 2008
www.atmos-chem-phys-discuss.net/7/S9507/2008/

Atmospheric Chemistry and Physics Discussions

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

H. Kokkola et al.

Received and published: 3 March 2008

We thank the Anonymous Referee 2 for good comments. Here are our replies to the specific points raised by the Referee.

Until a more rigorous analysis, particularly a 3-D analysis under reasonably heterogeneous conditions in which the simplified version of the model is compared with a non-simplified version of the model is performed, I would not recommend publication of this version of the paper. For the comparison, I would suggest selecting several points in the domain and comparing the aerosol number and composition size distributions after one day of simulation, at a minimum, between the more and less resolved cases. Emissions, advection, diffusion, and deposition, along with microphysical processes, should be allowed to affect the distributions. Since the module has been run in ECHAM5 (Section 3.5), 3-d simulations should not be a limitation.

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While a 3-D analysis would certainly give very interesting information on the performance of the aerosol module, it is - as the other reviewer (Ref 1) points out - a scientific study on its own. Such a study will be the next step of validating SALSA aerosol module but is out of the scope of this manuscript. In the current manuscript we aim to present the methods and design of the module (e.g. the varying bin width is a feature that has not to our knowledge been used previously) as well as the test how accurately our simplified approach captures the aerosol dynamic processes. We think it is important to test these processes separately from the processes controlled by the host model (advection, diffusion, emissions, often also deposition) because this way it is easier to identify the magnitude in the error of these different individual processes resulting from simplifications in our microphysics module. In a global simulation this becomes nearly impossible.

In the revised version of this manuscript we will present analyses with SALSA in more heterogeneous conditions and using more heterogeneous aerosol composition. We believe these additional tests will result in a more complete validation of the chosen approach in the scope of this manuscript.

Additional comments:

1. The divisions in Figure 1 will result in certain errors for certain compounds. Black carbon particles exist down to about 20 nm. The low cutoff for black carbon assumed appears to be around 51 nm. Organic carbon is abundant in coarse-mode particles from biomass burning, but such particles are neglected in the figure. A significant submicron component of mineral dust exists, but this is neglected in the figure. These simplifications indicate the model will perform the least accurately near combustion, biomass burning, and dust emission sources as well as downwind of these sources. The text states that these divisions can be "chosen differently for different types of simulations or model configurations," however global simulations require all these situations, Further, how is a user to know whether the configuration chosen is accurate

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without running a set of sensitivity tests? If the purpose of the paper is to present an optimized code that has been evaluated with respect to the simplifications, users should be able to rely on the code without performing their own evaluation. Otherwise, the evaluation performed in the paper is not meaningful. Thus, either the paper should demonstrate that the simplifications are generally applicable to most situations globally (through 3-D analysis) or the simplifications should not be advertised in the paper. In addition, the real size ranges of black carbon, mineral dust, and organic carbon should be discussed.

After the referee comments, we have come to the conclusion that it is important to include dust in size bins smaller that 700 nm to account for the radiative effects and ice nucleating effects of dust in the accumulation mode. The aerosol module has now been modified so that dust is now also present in the subregion 2 with low cut off size of 50 nm.

The black carbon is found in particles down to 20 nm but the mass in such small particles is very small.

The model setup presented in the manuscript is assumed to be the most suitable one for global scale simulations with the least amount of size bins and chemical compounds. The box model comparisons serve a purpose of showing that the individual microphysical processes are treated accurately enough despite of the simplifications. Also, the results presented in the manuscript are the cases which give the largest errors compared to more accurate solutions. If the host model can allow more size bins, the accuracy will naturally be improved.

Organics are included also in the coarse mode in the water soluble material. We will improve the description of subrange 3 particles in the text and Figure 1. The black carbon (BC) is found in particles down to 20 nm but the mass in such small particles is very small, which is why we have chosen the low cut off size for BC to be 50 nm. Nevertheless, we will add discussion on the real size ranges of BC,

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mineral dust and OC.

2. What is the analytical or numerical method of determining the width of each size bin? How are users of the code to know what regions of the world are "influenced less by microphysical processes?"

The size bins within a subregion are divided to have constant volume ratio. The default setup for the model presented in the manuscript should be applicable for global scale studies. Because of having no internal mixing in subregion 1 and the absence of black carbon in subregion 1, it can be expected that the model has inaccuracies for very high resolution simulations near the sources. We will add discussion about this in the revised manuscript.

3. Why were the numbers 51 nm and 730 um selected for representing cutoffs? Is this arbitrary?

The procedure to find optimal cutoff limits is given on page 17710, lines 5-15, but we will clarify the explanation of the procedure in the revised manuscript.

4. The underlying solution scheme for condensation provided in Equations 2 and 6-10, which combines the discretized aerosol growth equation with the discretized gas-aerosol mass conservation equation to solve for the gas and plug the result back into the aerosol equation, is the Analytical Predictor of Condensation scheme with a saturation vapor pressure set to zero, from Jacobson (Aerosol Science and Technology 27, 491, 1997, Eqs. 10, 12, 13) rather than from Lehtinen et al. (2004). The name of the original scheme and its source should be stated. If differences exist, they should be clarified.

This is correct. We will clarify this and include the reference to the APC method.

5. The concept of adding nucleation to this solution scheme by dividing the nucleation rate by the gas concentration (Eq. 11) appears to originate from Jacobson (JGR 107, D19, doi:10.1029/2001JD002044, 2002, Equation 33). This should be mentioned as

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well. If differences exist, they should be clarified.

This is also correct. We will include the necessary references and clarifications.

6. It is not clear how the authors are calculating the conversion from insoluble to soluble size bins. What is the physical basis for the method?

As stated on page 17712, when the insoluble particles gain enough soluble material so that they will activate at 0.5 %, the insoluble particles are transferred to the parallel soluble size bins. We will clarify this in the revised manuscript.

Here, we follow the method used in the microphysical module M7 in ECHAM5 in which the insoluble modes are combined with the soluble modes after enough soluble material has accumulated in the insoluble mode.

7. Figure 8. Does this figure show the sum of S(VI) and organics? Why are the two summed together into one line instead of shown separately?

The figure only shows the S(VI) concentration since the organics concentration was assumed to be equal to that of S(VI) (page 17721, lines 25-27).

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