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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 #2

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This paper describes an aerosol computer code and some optimizations and basic applications of it. Because several other aerosol codes used in 3-D models exist, the paper needs to rest on new science. The novelty of the new paper is stated to be that it reduces computer time by limiting the chemical compounds and physical processes available in different size sections of aerosol particles, divides the size distributions into sections of variable width, minimized the number of size sections, and treats only specific size physical processes in a given size bin. There is no discussion, however, as to whether other computer models have or currently apply some of these optimizations. Further, the analysis is limited to a few idealized box model cases and does not give an idea of how the simplifications will respond to the tens of millions of different situations in a 3-D model, particularly one where feedbacks may amplify some of the differences.

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.

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 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.

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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?”

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

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.

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 well. If differences exist, they should be clarified.

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?

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?

8. It seems that this paper has a lot of authors (12) for a process-box-model analysis. Did all authors contribute substantively to the paper? If not, I would suggest that the number of authors be reduced to reflect those that did substantive work on the paper. If so, please ignore this comment.

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Interactive Comment