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# *Interactive comment on* "Technical Note: A new SIze REsolved Aerosol Model (SIREAM)" *by* E. Debry et al.

**Anonymous Referee #1**

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Referee Comment on "Technical Note: A New Size REsolved Aerosol Model (SIREAM)" by E. Debry, K. Fahey, K. Sartelet, B. Sportisse and M. Tombette.

This paper presents a detailed description of a bin-resolved aerosol model which solves that part of the General Dynamics Equation (GDE) concerned with clear-air processes (i.e. aerosol nucleation, coagulation and condensation/evaporation).

The paper presents a very thorough description of the physical parameterizations and numerical techniques used in the SIREAM model. A brief description of the cloud and heterogeneous processes which are not directly part of SIREAM is also included.

On the whole the paper is well written and the detailed numerical methods employed by the model are mostly clearly described.

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My main criticism is that the methods presented are often not sufficiently well referenced.

For instance in section 3.2.1, the coagulation equations use the method described in Jacobson et al (1994) except that the time integration is carried out as 2nd order explicit (described in section 3.3.1) rather than the semi-implicit formulation in Jacobson et al (1994).

Also, the section describing the aqueous chemistry in clouds (2.3.1) is at times unclear and needs substantial clarification (see specific comments).

However, the detailed description of the parameterizations and numerical methods used in the model is to be welcomed and the Technical Note will be a worthy addition to ACP once the specific comments have been properly addressed.

Specific Comments —————

1) Abstract – page 11846, lines 5-7 – state clearly that the hybrid method refers to a hybrid equilibrium/dynamic approach to mass transfer.

2) Introduction — page 11847 line 8 – replace "rigorous" with "detailed". Simpler models can still be rigorous.

3) Introduction — page 11847 lines 15-20. The authors should include references to the other sectional models in the literature (see e.g. Jacobson, 1997; Adams and Seinfeld, 2002; Gong et al, 2003; Spracklen et al, 2005) and explain how the parameterized processes and/or numerical treatment described in the paper differs from these.

4) Introduction — page 11847 line 22 — do the authors really mean SIREAM is "strongly coupled" to MAM or are the two models rather alternative aerosol models within "ATMODATA".

5) Introduction — page 11847 line 26 — the paper should state whether the model is intended for use in Lagrangian or Eulerian (global or regional?) frameworks.

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6) Section 2.1 – page 11849 line 10 – after the description of the method for determining the inorganic composition, the method for determining the organic composition should be included (i.e. add a sententence stating that the SORGAM scheme is used [with reference to the Schell papers]).

7) Section 2.2 – page 11850 line 16 – the expression used to calculate the coagulation kernel should be stated here — it is not explained anywhere in the paper.

8) Section 2.2.3 – page 11851 line 11, Section 2.2.4 page 11852 line 9 — the Nenes et al (1998) reference for ISORROPIA should be included here for easy reference.

9) Section 2.3 - page 11857 line 14, – the authors state that the processes are not related to the General Dynamics Equation. But this is not really the case since both will affect the size distribution. Better to state that the processes are not part of SIREAM.

10) Section 2.3 – page 11857 line 17, – this section should explain how these other processes are treated in the model with respect to the main SIREAM code – are they solved process-split? The explanation could go either here or perhaps better in section 3.1.

11) Section 2.3.1 – page 11858 line 5 and 13. — I strongly agree with the comments of the other referee regarding the use of a critical activation diameter of 0.7 microns. The description suggests that bins larger than this (dry?) diameter be mapped on to a monomodal (lognormal?) distribution with median diameter 0.4 microns. This seems inconsistent to me — please can the authors clarify.

12) Section 2.3.1 — page 11858 lines 13/14 — it seems strange that there is little sensitivity to the choice of the size distribution assumed for the activated particles. The two references given for the aqueous chemistry (Fahey, 2003; and Fahey and Pandis, 2001) are also not easily available, so the reader is not able to understand why this is so. Please can the authors explain.

13) Section 2.3.1 – page 11858 line 15 – the authors should state (as in Sportisse et

al, 2006) that for the prupose of cloud-chemistry, the cloud droplets are assumed to have a fixed size of 20 micron dry diameter.

14) Section 2.3.1 — page 11858 lines 17/18 – it is not clear to the reader which parts of the mass transfer to the cloud droplets is solved dynamically and which is solved assuming equilibrium. I suggest that the authors include Table 1.2 [page 22] in Sportisse et al (2006).

15) Section 2.3.1 – page 11858 lines 26/27 — this sentence is not clear — Sportisse et al (2006) describe the diphasic model that is used. This should be briefly described if used along with a clear description of the mapping of the cloud droplet distribution onto the particle distribution.

16) Section 3.1 – page 11860 lines 8/9 — I strongly support the other referee's comment that when the redistribution onto a fixed size grid is used (as required in a 3D model), SIREAM is effectively using the quasi-stationary method of Jacobson (1997) rather than a moving-sectional method. Furthermore I do not think it is entirely accurate for the authors to state that the method avoids numerical diffusion. The quasi-stationary method will avoid *some* of the numerical diffusion associated with the full-stationary method but will not perform as well as the moving-centres or full-moving methods, as shown by Jacobson (1997). The authors should revise their statements accordingly (and other places this is stated in the text – (heading page 11862 line 2).

17) Section 3.2.2 — page 11862 line 2 – section 3.2.2. needs an opening paragraph to introduce this section as it provides the main new method in the paper. The two alternative methods should be clearly introduced and I agree with the other referee that once again, since the 2nd method is essentially the quasi-stationary method as in Jacobson (1997) it should be referenced and stated as such. The text should also describe in which situations each of the methods should be used (with advantages/disadvantages).

18) References: page 11873 – the Koo et al (Aer Sci Tech) paper should have authors Koo, Gaydos and Pandis — please amend accordingly.

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19) References: page 11874 – the Sartelet et al (Aer Sci Tech) reference was published in 2006 – plesae revise accordingly.

References not in manuscript ——————————-

Adams P. and J. Seinfeld, 2002, Predicting global aerosol size distributions in general circulation models, JGR Atmos, 107, 4370, doi:10.1029/2001JD000483.

Gong S. et al, 2003, A size-segregated simulation of atmospheric aerosol processes for climate and air-quality models 1: Module development, JGR Atmos, 108, 4007, doi:10.1029/2001JD002002.

Jacobson, M. et al, 1994, Modelling coagulation among particles of different composition and size, Atmos. Environ., vol. 28, no. 7, pp. 1327–1338.

Jacobson, M., 1997, Development and application of a new air pollution modeling system, Part II: Aerosol module structure and design, Atmos. Environ., 31A, pp. 131-144.

Spracklen, D. et al, 2005, A global off-line model of size-resolved aerosol microphysics: 1: Model development and prediction of aerosol properties, Atmos. Chem. Phys., 5, pp. 1-26.

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Interactive comment on Atmos. Chem. Phys. Discuss., 6, 11845, 2006.

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