

Interactive comment on “Technical Note: A new Size REsolved Aerosol Model (SIREAM)” by E. Debry et al.

e. debry

debry@cerea.enpc.fr

Received and published: 13 December 2006

REPLY TO REFEREE COMMENT S4923

We thank the reviewer for her/his positive comments on our paper!

We repeat the reviewer's statements below and comment on them:

1) Please could the authors add a brief paragraph to the introduction summarising which programming languages SIREAM has been written in, as well as dependancies on other packages (and which versions).

SIREAM is written in fortran77. Its external dependencies are the thermodynamic module "ISORROPIA" and the VODE solver from the ODEPACK ordinary differential

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

equation package, the "double precision" of this solver is required. The current version of ISORROPIA is 1.7, but any version since 1.5 is compatible with SIREAM. This module is not distributed with SIREAM itself and has to be retrieved by the user on the ISORROPIA website (<http://nenes.eas.gatech.edu/ISORROPIA/>). As far as we know the VODE solver is in the public domain so that we freely distribute it with SIREAM. Nevertheless the user may retrieve his or her own version from the url <http://www.llnl.gov/CASC/software.html>. I propose to add one section "Implementation" just before the conclusion in order to answer all questions with respect to the computer implementation and its details.

2) I note that SIREAM has been used by Sartelet et al. (2006) for comparison with the modal model MAM in a couple of mass-transfer testcases, however the only settings for SIREAM that they list are the number of bins. As no testcases have been given in this paper I believe it would be helpful for the reader if the authors were to include the lower two panels of Figure 2 from Sartelet et al. (2006) and a short paragraph on which of the settings detailed in this paper were used in the testcase.

The SIREAM settings in Sartelet et al. (2006) are not clearly indicated, such as, for example, the choice of redistribution algorithm. Nevertheless I am not sure it is a good idea to add in this technical note any missing information in article Sartelet et al. (2006). To obtain any missing details it is preferable to contact Karine Sartelet .

3) In the abstract and the first paragraph of Section 3.1 the authors describe the numerical method used for solving condensation/evaporation as a "moving sectional" approach. This description does cover the method used to calculate the new particle sizes after such growth, but makes no reference to the redistribution of particles on to a fixed size grid which is necessary for using such a method within a 3-D model. As the redistribution process is integral to the method I believe that a more appropriate description would be "quasi-stationary" (as used by Jacobson, 1997). Please could the authors change these two uses of the phrase "moving sectional" to either "quasistationary" or another such suitable label.

We replace "moving sectional method" by "quasistationary method (Jacobson, 1997)".

4) p. 11846, line 7: The description "hybrid method" is a little vague. Please could the authors expand this; I would recommend "hybrid equilibrium/dynamical mass-transfer method".

We will replace "hybrid method" by "hybrid equilibrium/dynamical mass-transfer method".

5) Having not studied cloud activation schemes I am a little confused by the first two paragraphs of Section 2.3.1. In the first paragraph the default value of the critical diameter for cloud activation is given as $d_{\text{activ}} = 0.7 \mu\text{m}$. In the second paragraph, however, it is stated that the activated distribution has a median diameter of $0.4 \mu\text{m}$. Are these values correct? If so please could the authors state if are they dry or wet diameters and better explain how they are related?

Both are dry diameters. I checked that the values are correct. The point is that there is no relation between them. The "dactiv" dry diameter is related to the aerosol size distribution, only bins with a dry average diameter greater than dactiv will be activated. The median diameter refers to the cloud droplet distribution which is parameterized independently from the aerosol. In the VSRM model developed by Kathleen Fahey the cloud droplet distribution is modeled as a bimodal (lognormal) distribution. Each mode is characterized by its mean dry diameter and its standard deviation. In order to reduce the computational burden, we only use one modal distribution. I propose to replace "critical diameter" (line 6, page 11858) by "critical dry diameter", then to replace the second paragraph (between line 11-15 included, page 11858) by the following one:

"The activated particle fraction is then incorporated into the cloud droplet distribution which is parameterized independently from the aerosol. In the VSRM model the droplet population is described by a bi-lognormal distribution. In order to reduce the computational burden, we only use one lognormal function whose mean dry diameter and standard deviation are respectively $0.4 \mu\text{m}$ and 1.8 (adim). The tests ... particle frac-

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

tion."

6) p. 11858, line 25: The default pH value for the cloud droplets is given as 4.16. Please could the authors explain why is it this value, and is an accuracy of three significant figures justified?

We replace "4.16" by "4.5" which is just a default pH value for cloud droplet when the electroneutrality algorithm fails. This value is a rough average of cloud droplet pH values found in (Pandis & Seinfeld 1998) or (Prupacher & Klett, 1997), most of them lies between 4.0 and 5.0. This value has no meaning in itself. Normally we would stop the simulation when the electroneutrality calculation fails but this is unrealistic for long simulations, for which we prefer to use a default value.

7) In Section 3.2.2 (pages 11865-11866) two methods are described for redistributing the particle distribution onto a fixed size grid after condensational growth. I believe the first is from Debye (2004), while the second is the same as the quasi-stationary method used by Jacobson (1997). It would help the readers understanding of the scientific context of these methods if the authors were to include references in their paper for other examples of these methods being used within the published literature.

The second method was developed to avoid the drawback of the first one as pointed out in the next comment. We did not realize that our development gave the same formulas as the method used by Jacobson (1997). We will detail in a few lines the development of the second method and explicitly cite (Jacobson, 1997).

8) It would be very useful to be able to ensure that both the aerosol mass and number are conserved during the redistribution of the particle distribution. However it does seem to me that, while it does this, the first of the redistribution methods (page 11865) is also very unstable. By redistributing equal proportions of the particle number and mass distributions within a Lagrangian bin, to bins on the fixed size grid, you set the average mass of each of the new bins to that of the original Lagrangian bin. So, while the average mass of one of the new bins will be within the boundaries of that bin, the

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

Interactive
Comment

average masses of the other bins will all be outside of their boundaries. In the middle of the aerosol distribution this effect is likely to be mitigated by the addition of mass from the other Lagrangian bins. However such mitigation will not occur at the edges of the aerosol distribution, and so the average masses in these bins will be unreasonable. Please could the authors explain how they have overcome this problem?

We will rewrite this paragraph to show that both methods conserve both mass and aerosol number, and highlight the differences between them. With the first method mass fraction and number fraction are the same so that the aerosol diameter is also conserved, which may result in the drawback pointed out by the referee. Nevertheless up to now we did not experience severe instabilities with the first method.

Technical Comments:

- 1) The reference to Koo and Pandis (2004) should be to Koo, Gaydos, and Pandis (2004).
- 2) The journal and page numbers for Stockwell et al. (1997) are 102 and 25847-25879 respectively.

Thanks for spotting these errors in the bibliography, we will correct them.

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

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)