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Geoscientific Model Development Discussions

Interactive comment on “Aerosol-climate interactions in the Norwegian Earth System Model – NorESM” *by* A. Kirkevåg et al.

Anonymous Referee #2

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General comments:

The article reports on the representation of aerosols and aerosol-cloud-radiation interactions in the CAM4-Oslo/NorESM earth system model. The applied methods are described and an evaluation of model results is presented. The manuscript thoroughly describes a number of innovative further developments such as improved representations of primary and secondary organic aerosol, sea salt aerosol, aerosol sedimentation and wet removal. The results of comparisons of simulations with measurements are highlighted in remarkable detail and possible reasons for discrepancies are discussed. It is quite valuable that the effects of different development steps are explored by means of targeted sensitivity simulations. The results of this analysis are an excellent basis for interpreting deviations between model and observations and even for understanding the role of individual aerosol constituents and specific processes in the climate system. For these reasons, the article is of high value for the climate modelling community.

Since the major focus is the description of new model features and the evaluation and interpretation of the resulting new model results, the paper is well suited for publication in GMD. The paper is generally of good technical quality. It is well written and well organized. Model concepts and evaluation results are clearly presented. Relevant literature is referenced thoroughly. I recommend publication after the following minor comments have been addressed by the authors.

Minor comments:

1.Page 2600, line 23: Explain which forcing is meant here (total aerosol, anthropogenic aerosol,. . .?)

2.The authors discuss that their model developments result in forcings which are closer to the AeroCom median or IPCC AR4 best estimates than the results of previous model versions. I think this could be misunderstood as a hard quality criterion. It is probably not really clear whether the AeroCom or AR4 results could be regarded as a reference since individual models could be superior to ensemble means when compared to measurements. I would recommend mentioning this in the text to prevent from misinterpretation. If the authors feel that, in some cases, the AeroCom or AR4 results could be regarded as benchmark, this should be justified in the paper.

3.Page 2603, lines 6-8: The authors mention that they do not discuss the semi-direct effect in this manuscript. It would be interesting for the reader why this discussion has been omitted.

4.Page 2608, line 9: The meaning of ‘thus’ is unclear to me. The fact that modes are changed according to processes does not imply to use size-segregated schemes.

5.Section 2.1, introduction on pages 2607-2609: It should be explained in more detail

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how the processing and transformation of the different aerosol species is realized in the model. It should also be motivated why the described approach has been chosen and what the advantages and disadvantages are when compared to other methods as the log-normal approach. It is not clear to me why the aerosol components are classified according to single processes while being subject to a number of different kinds of processes. As an example, if SO4/OM/BC(coag) is formed by coagulation, it is not clear why it should not be subject to condensation of SO4. Or can aerosol mass be present in different components at the same time in the model? It would be helpful to the reader to find some brief information in the manuscript to avoid consulting previous literature. It should also be mentioned how gas phase precursor chemistry is realized (a short description with additional reference to section 2.1.6 would be sufficient).

6.Page 2616, line 1: the compensating effects should be described in more detail.

7.Section 4.1, Table 3: The reasons for differences between ‘total emissions’ and ‘total sources’ in the case of primary particles should be discussed. I would expect the two quantities to be identical.

8.Section 4.2.2: To learn more about the vertical transport of aerosol in the model it could be useful to consider also observed vertical profiles of primary particles, which are not subject to in-situ generation from precursors and therefore largely constrained by transport and deposition. Data on primary aerosol vertical distribution from aircraft-based field campaigns is available for black carbon (see e.g. Schwarz et al., 2008, JGR, D03203; Schwarz et al., 2006, JGR, D16207) or dust (see e.g. Weinzierl et al., 2009, Tellus B, 61, 96–117).

Editorial changes:

1.Figure 1, caption, line 7: ‘assumed transformed’ makes no sense. Q(xx) should be explained.

2.Page 2608, line 6: replace ‘is’ by ‘are’.

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3.Page 2630, line 23: replace ‘data prepared by Brigitte Koffi for year 2007’ by ‘data for 2007 (B. Koffi, personal communication, ...)’, or similar.

4.Section 4.3: the notation of units (W/m2) is partly erroneous.

5.Page 2651, line 14: Rewrite ‘other changes have proven important’.

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