

Interactive  
Comment

## ***Interactive comment on “Simulation of aerosol optical thickness during IMPACT (May 2008, The Netherlands) with ECHAM5-HAM” by G.-J. Roelofs et al.***

### **Anonymous Referee #1**

Received and published: 14 April 2010

This manuscript reports simulation results of a coupled aerosol-climate model, ECHAM5-HAM, over an observation site (Cabauw) in the Netherlands during May 2008. The authors attempted to evaluate their model by comparing the simulation results with in situ and ground-based observations obtained during an intensive observation period (IMPACT). Such efforts of evaluating numerical models with detailed aerosol optical and physical properties are important to reduce the large uncertainty associated with diverse effects of aerosols on the climate system, which warrant publications of papers of this kind. However, I have several concerns that should be addressed or resolved before this manuscript can be published in ACP.

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(Mandatory) As this study focuses on comparing model outputs with point-wise measurements, it would be necessary to explicitly discuss their comparability (e.g., representativeness of point measurements over a larger model grid box). Also, size of model grid box and temporal resolution(s) of the feedbacks between aerosols, chemical components, and meteorology should be mentioned, rather than assuming that readers would know such information. In addition, it should be discussed in the manuscript that how authors try to quantitatively compare model outputs with ground-based or in situ measurements to reduced mismatch of target volumes between them (e.g., averaging AERONET AOT for 0.5~2 hours to be comparable with model output).

(Highly Recommended) Although the focus of this manuscript is evaluating the performance of ECHAM5-HAM using suite of measurements available at a single site, two dimensional distributions of aerosols need to be provided. In that way, readers would better understand the circumstances around the site during the study period. I would like to strongly suggest authors should consider including maps of AOT or other relevant variables from the model in addition to satellite-retrieved AOT maps around the region and show the location of the ground site.

(Minor) Page 1, L31 – Page 2, L2: There are also significant uncertainties in satellite-based aerosol retrievals. Aerosol data from remote sensing are usually biased toward the clear-sky condition. Temporal and spatial inconsistency between the properties/quantities of aerosols and clouds result in uncertainty in estimating aerosols effects on clouds and climate.

(Minor) Page 3, L9-10: CCN concentration is not necessarily related to AOT through a bijective function. Large scatter can show up if one compare them depending on aerosol optical and physical properties and their vertical distributions. I wonder what the point of this statement is.

(Mandatory) Page 4 – Model description: Authors seem to assume that readers know about model grid size (i.e., horizontal resolution) and temporal resolutions.

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(Highly recommended) Page 5, L30: It appears that there are Level 2.0 (quality assured) AERONET data are available for Cabauw site, but authors used Level 1.5 data. For validation purpose, it is highly recommended that users use Level 2.0 data.

(Mandatory) Page 5, L20-26: Aerosol chemistry/transport models deal with physical quantities (e.g., mass) of aerosols while remote sensing deals with their optical properties/quantities (e.g., AOT). Therefore, there are uncertainties in converting aerosol mass into extinction coefficients (thereby AOT), which is especially large for dust and smoke (e.g., Reid et al, 2005 [J. S. Reid, T. F. Eck, S. A. Christopher, R. Koppmann, O. Dubovik, D. P. Eleuterio, B. N. Holben, E. A. Reid, and J. Zhang, A review of biomass burning emissions part III: intensive optical properties of biomass burning particles, *Atmos. Chem. Phys.*, 5, 827-849, 2005]; Hand and Malm, 2007 [J. L. Hand and W. C. Malm (2007), Review of aerosol mass scattering efficiencies from ground-based measurements since 1990, *J. Geophys. Res.*, 112, D16203, doi:10.1029/2007JD008484). Authors didn't explicitly mention how such conversions were made. They should have mentioned that any influence or implications regarding this matter.

(Minor) Page 7, L24-30: IWV could be correlated with AOT coincidentally when transported aerosols happen to be transported with moist air before they get to the location of interest (observation site).

(Mandatory) Page 8, L3: The website referred was not accessible. Please provide more details about the surface measurements.

(Mandatory) Page 8, L19: Please spell out ECN. Also, in other parts of this manuscript, there are several acronyms, which are not spelled out or explained clearly when they are first introduced in the text.

(Minor) Page 9, L20-21: The caption for Figure 5 says that the profiles are averaged over May 2-14. Which one is right? Please clarify.

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(Highly Recommended) Page 9, third paragraph (discussion for Figure 5): The comparison provided in the Figure 5 doesn't seem to be apple to apple. During the period of average (1/2?-14 May), there seemed to be changes in aerosol properties (as indicated by changes in Angstrom exponent and AOT), which might be associated with changes in vertical profiles. Authors may select a few episodes with aircraft measurements, and then compare them with model outputs at the time of selected flights. Or, at least, an average profile of model outputs at the time of respective flights should be compared.

(Mandatory) Page 10, L7-20: Again, while plots show qualitative comparisons (time series at different spatio-temporal resolutions), quantitative differences between model and measurements are provided in the text. It should be provided how model outputs and measurements are matched up with each other for such quantitative comparisons.

(Highly Recommended) Page 11, L1-6: This is interesting statements. More detailed descriptions about the processes and/or parameterizations in the model are highly recommended.

(Highly Recommended) Page 11, L18-20 & Page 16, L4-6: The efficiency of vertical mixing affects vertical distributions of water vapor. I wonder how it affected column water vapor. Please explain.

(Mandatory) Page 13, L25-28: Dust mass scattering/absorption efficiencies are uncertain. Even when simulated aerosol mass were perfect, "simulated AOT" could be higher or lower than observed, depending on such conversion. Again, authors need to discuss more details on how simulated aerosol mass was converted to optical properties (extinction coefficients or AOT).

(Highly Recommended) Figure 3: Are the observed surface PM10 daily averages or instantaneous? It is hard to compare model outputs with observations from Figure 3. For instance, on May 26, a sharp change in PM10 was simulated, and average of simulated PM10 might be close to the observed, but hard to compare them. If the

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observed PM<sub>10</sub> is daily averages, I would suggest authors to show daily averaged PM<sub>10</sub> from simulations.

(Minor) Figure 4: It is not clear whether the two observations were made at the same location or not.

(Mandatory) Figure 5: What do dashed lines stand for? It should be included in the figure caption.

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