

Interactive comment on “Implementation of a new aerosol HAM model within the Weather Research and Forecasting (WRF) modeling system” by R. Mashayekhi et al.

Anonymous Referee #2

Received and published: 13 October 2009

General comments

This study presents an application of the implementation of the aerosol HAM model within the regional Weather Research Forecast/Chemistry WRF-CHEM model, over a domain with a 30 km grid horizontal resolution centered over Turkey, and covering South Western Asia, Northern Africa and part of Europe. Descriptions of the main features of the coupled system, together with the microphysical core M7 are provided. Two six-day time periods, 22-28 February 2006 and 6-12 May 2006, are simulated with the WRF-HAM system. First results are presented focusing on the aerosol-radiation feedback of major aerosol compounds, comparisons with observations for daily aver-

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aged aerosol mass concentration of SO₄ (available from EMEP sites), diurnal variation of PM₁₀ and shortwave fluxes measured in Tehran.

The work presented in the paper addresses the importance of gaining a better understanding of the aerosol-radiation interaction and its implication for weather prediction, particularly over a geographical region, the Middle East, which is generally poorly monitored.

The current manuscript is nicely written, and the overall presentation is well structured. However, in agreement with the first referee, the authors do not justify the relevance and the implications of the work presented here. The aim and the scientific questions the authors want to answer are not clearly stated. In addition:

- The methodology description is not always sufficiently complete and precise to allow the results reproduction by fellow scientists. It lacks of complete description of the different processes implemented in the model (see first referee's comments), including detailed descriptions of the emissions adopted in the study. Observations used for comparisons should also be presented in more detail.

- Some results need to be checked for consistency (see first referee's comment for Figures 1 and 2). Comparisons with measurements are too qualitative, and there is very little discussion with findings in the relevant literature.

- There are not concluding remarks and discussions of the work and its implications at a scientific level.

Some of these issues have already been addressed in the response of the authors to the first reviewer's comments. Therefore, here I will indicate those important aspects that need to be further considered and adequately stated or analyzed.

I cannot advise the publication of the present manuscript, unless these issues, described in detail below in the list of the specific comments, will be properly addressed.

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I would ask the authors to address the following:

1. Title

The current title is misleading (is the aerosol HAM model a new aerosol model?), and does not clearly reflect the content of the paper. Please follow the first referee's suggestion including the area considered for the study and the main scientific contribution of the manuscript. e.g. The aerosol-weather forecasting modeling system WRF-HAM: Analyses of the radiative effects of primary aerosol impacts over Middle East.

2. Abstract

It needs to be more concise. It should describe the major features of this study, including the geographical area considered clearly mentioned. In addition, an overall descriptions of the observations used for comparisons with the model results should be provided. Only results representing a clear advance in the field of study should be mentioned (see first referee's comment).

3. Introduction

The HAM model is a simplified primary aerosol mechanism commonly adopted for global applications, while the WRF-CHEM model is a regional weather/chemistry model based on the not-hydrostatic WRF model. In this light, how can the usage of HAM be justified in the WRF/Chem model? What are the assumptions and the implications of this choice, and, why should the HAM module works better than the other models? Therefore, in agreement with the first reviewer, I ask the authors to extend the introduction including:

- a clear justification of the coupling,
- the research questions they would like to answer,
- the main objective of the development.

If the authors aim to investigate the performance of the WRF-HAM system, then a

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comparison with relevant literature is required.

4. Methodology

In their reply to the first reviewer, the authors already addressed some points on the model description, model configuration, and emission section .

For the emission section I suggest to present and discuss it as in Stier et al. (2005), and possibly to add a table such as table 2 (in Stier et al (2005)). In the emission section, the authors should also mention what kind of temporal profiles they use for emissions, and the implications of using constant rates (e.g for PM10).

I ask the authors to include in the manuscript some explanations on the implications of using emissions of 2000 for a 2006 simulation.

I ask the authors to include examples of maps of the mineral dust emission distributions for the two time periods chosen in their simulations. These maps could help the reader to better understand the results discussed by the authors.

I ask the authors to include a section dedicated to the observations. The section should provide the reader with more pieces of information about the measurements and their site locations (e.g. for PM10 and shortwave radiation fluxes measured in Tehran, little information is currently provided).

5. Results

Please check the results/model configuration for Figures 1 and 2. I recommend the authors to compare their results with the scientific literature and report their findings on the manuscript.

I ask the authors to provide more quantitative analyses for the comparisons between their observations and model results (tables with basic statistical analyses, e.g. Pearson correlation coefficients, regression lines and bias values, should be reported). This is a requirement to assess the performance of a coupled system such as the WRF-

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HAM system. Qualitative analyses are not sufficient to adequately judge the system performance.

Figure 4, c) and d): Unless the modeled mean PM10 compositions are compared with the corresponding mean observed compositions, I can hardly see how this part can be used to deduce conclusions as mentioned in pg 694, ln 19-22.

6. Conclusions and Outlooks

I agree with the first reviewer that there are not concluding remarks and discussions of the work and its implications at a scientific level.

Technical issues

In addition to the first reviewer's comments:

-pg 692 ln 4 'fist'-> 'first'

-Please delete ln 19-22 in pg 694 (See note for Figure 4 in point 5 of the previous Specific comments).

Reference:

Stier, P., Feichter, J., Kinne, S., Kloster, S., Vignati, E., Wilson, J., Ganzeveld, L., Tegen, I., Werner, M., Balkanski, Y., Schulz, M., Boucher, O., Minikin, A., and Petzold, A.: The aerosol climate model ECHAM5-HAM, *Atmos. Chem. Phys.*, 5, 1125–1156, 2005, <http://www.atmos-chem-phys.net/5/1125/2005/>.

Interactive comment on *Geosci. Model Dev. Discuss.*, 2, 681, 2009.

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