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Discussions

Interactive comment on "The regional aerosol-climate model REMO-HAM" *by* J.-P. Pietikäinen et al.

Anonymous Referee #2

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General Comments

The paper describes the regional aerosol-climate model REMO-HAM and a comparison of a) model results with different spatial resolutions, b) with results of a global climate model ECHAM5-HAM, and c) with observations. It is well written and with a few exception easy to follow. Nevertheless, I came to the conclusion that in its current status the paper should not be published in GMD. The model system is based on a hydrostatic model and for physical reasons it is therefore limited to spatial resolutions of 10 km. This is the reason why the German weather service switched to a non-hydrostatic model system almost a decade ago. As we cannot expect that a hydrostatic model system can be used to describe the atmospheric processes on the regional scale properly and especially is not able to perform convection permitting simulations on the regional

scale in cannot be expected that the knowledge gained with this model system goes far beyond what is already know from simulations with ECHAM5-HAM. It is hard to understand that methods that are used in ECHAM5-HAM are taken over one to one to describe the interaction of aerosol particles with clouds on the regional scale. It is well known that the chemical composition of the aerosol particles determines their size distribution and their ability to form cloud droplets and ice crystals. The simulation of the chemical composition of the aerosol particles requires a detailed chemistry module for gas phase reactions which is missing in the model. I was surprised that the authors, some of them are worldwide known experts in atmospheric chemistry are satisfied with prescribing (monthly mean) fields for OH, H2O2, and ozone on the regional scale and account for sulphur chemistry only. It is also astonishing that nitrate as which is an important contributor to the aerosol mass is neglected within a regional scale model. Organic chemistry which contributes up to 50 % of the chemical composition of the particles is totally neglected. Both is no longer state of the art. It is also not so easy to follow the argument that boundary layer nucleation was neglected because it produced to high particle numbers in comparison to observations. As the authors partly mention these shortcomings and refer to future studies with the model system I would propose to wait with the publication of the model system until it has reached a competitive stage to other model systems that are documented in literature (e.g. Forkel et al., 2011).

The authors claim that REMO-HAM at its current stage gives reasonable results in comparison with observations. Based on the results that are presented I do not see that reasonable agreement.

Specific comments:

The paper is in general well written. However, throughout the model description and the discussion section the authors quite frequently postpone an explanation to a forthcoming section. This is a little bit boring. It forces the reader to switch between different sections when trying to understand the paper. Examples are pages 751, 752, 754.

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page 739, starting at line 10: The agreement with the observed number concentrations is far from being reasonably well simulated.

page 741, line 23: What means ECHAM-HAM includes the aerosol microphysics HAM as well as M7. Are these different modules to treat aerosol dynamics? Which one is used within the study?

page 742, line 25: Please comment on the quality of the Tanre et al. (1984) aerosol climatology over Europe. See for example Zubler et al. (2011).

page 743, line 17: Which of the two water uptake methods is used?

page 744, line 10: How is the NO3 concentration calculated in REMO-HAM?

Section 2.3.1: It is astonishing that the authors always use the same spatial resolution of the emission data. This has consequences for the interpretation of the model results. How is the weekly and the diurnal cycle of the emissions treated in REMO-HAM? If there is no treatment why should the model results agree with observations?

page 749, line 6: Please explain why only the binary sulphuric acid – water based scheme is used. Later on you describe that no boundary layer nucleation scheme is used. Why not?

page 751, line 6: I cannot see that the modelled values are fairly close to the observations. The opposite is true keeping in mind that a logarithmic scale is used. Moreover I was surprised that in case of Hytiälä and Mace Head REMO-HAM even gives less good results in comparison with ECHAM5-HAM. Please explain the reasons.

page 754, line 24: Figure 8 shows that all model versions used in the study are overestimating the measured SO2 concentrations tremendously. This might be due to wrong emissions or due to wrong OH concentrations to give two reasons. In any case it questions the quality of the aerosol concentrations.

page 755, line 14: I am confused by the statement that the differences are due to

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different resolutions of the emissions. In the emission sections you explained that you always use the course emission data.

page 757, line 23: At this point the reader is informed that the radiative effects of the aerosol particles are not taken into account. This information should be given already in the model description. page 758, line 11: Please explain the resolution scaling factor in more detail.

Table 1: Explain the meaning of S, BC, POM etc.

Figures 1 and 2: In my opinion figure 2 is not necessary. The information given there could be transferred to Figure 1. Please explain the placement of the area with the highest resolution used by REMO-HAM. Is the selection of the area due to scientific reasons?

Figures 5, 6: I do not see the necessity to present these figures, as no observations are presented.

Figure 9: Please give a definition of a sulphate production flux. What is meant by this quantity.

Figure 11:

Comparing the results of REMO and REMO-HAM it is quite interesting to see that including the aerosol cloud feedback leads an increase in 2 m temperature. Please give physical reasons for that warming.

Interactive comment on Geosci. Model Dev. Discuss., 5, 737, 2012.