

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

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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 Nevertheless, I came to the conclusion that in its current status the paper should not be published in GMD.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 de-

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scribe the atmospheric processes on the regional scale properly and especially is not able to perform convection permitting simulations on the regional scale cannot be expected that the knowledge gained with this model system goes far beyond what is already known 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, H<sub>2</sub>O<sub>2</sub>, 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 too 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.

We thank the reviewer for valuable comments for improving our manuscript. Throughout the text reviewers comments are marked with boldface and after each comment

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follows our reply.

We have merged Figures 1 and 2 so the numbering is changed in the corrected version starting from the Figure 2 (previously Figure 3). In addition, the Figures 3, 6 and 7 (new numbering) have been updated.

Our meaning was not to mislead the reader to think that REMO-HAM can go to resolution below 10 km. We thought this would be clear when we mentioned that the model is hydrostatic (as the reviewer points out as well, hydrostatic assumption is only valid down to 10 km resolution). The aim of the current work was to present regional aerosol-climate model which can be used to simulate most of the main aerosol processes on 10-50 km scales. REMO does have a non-hydrostatic extension to the hydrostatic core, but this version was not used in this study. ECHAM, being a global model, is quite heavy computationally when aerosols are included. This means that 50 km resolution simulations are in no way reasonable at this stage of the model development. REMO-HAM is a new version of REMO model, which fits perfectly to these scales, and is a well known and validated model. The definition of regional scale, can of course vary, but 10-50km resolution is still widely recognized to be part of it (for more info, please see for example <http://www.meteo.unican.es/en/projects/CORDEX> and forthcoming IPCC AR5 regional climate projections).

It is true that the 10 km simulation have pushed the model to its limits, for example the convection parameterization (this was also mentioned by the reviewer). Still, we wanted to show that the model can be used with aerosols on this scale (for longer term simulations, meaning more than couple of months). It is also true that one should be careful when taking modules from a global model to a regional model. In this case, we do believe that the resolution jump was not too high and the results from our comparisons with ECHAM and with measurement data clearly indicate that the physics does work quite adequately. We did use scaling parameters for the clouds (the reviewer has a question later on about this) so the implementation was not done one to one, but the resolution difference was taken into account.

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The chemical part of the model is quite simple. There is a version of REMO where the RADMIII chemistry model has been coupled with it. The problem with models is that more detailed processes increase the computational burden. The approach in this work has its own deficiencies, but has been shown to give reasonable results from global scales to regional scales (used in many of the models cited in this work). Again, the aim of this work was not to present process scale model (that is, scales below 10 km resolution), but to present regional climate-aerosol model valid for 10-50 km resolution simulations. Nevertheless, the reviewer is right that this simple chemistry has quite a lot of effects to aerosol properties (especially on finer resolution). We are currently improving the chemistry part of the model. This ongoing research looks promising and will improve the chemistry part on both, global and regional scales.

The points to nitrate chemistry and SOA model are absolutely true. Nitrate chemistry is still unfortunately rare in climate models. Work with SOA implementation is an ongoing project (as well as coupling with radiation). We do feel that it is important to show that REMO-HAM's core does work at this stage and gives reasonable results. In addition, we will try to clarify more better why did we not use BL nucleation (the problem with too high SO<sub>2</sub>). Nevertheless, we do not see the point of waiting these improvements, since the model is usable regional aerosol-climate studies in its current state. For example, the connection between clouds (better presentation of cloud droplet and ice crystal concentrations) is already on its own a big improvement for the model. Moreover, from the aerosol point of view, the model is used for black carbon studies in its current state (soon to be published).

#### **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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This is true. We have tried to simplify the text. The problem is that in some parts, the explanation needs more info and can be explained only later.

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

We, of course, see the agreement to be reasonably good. Still, the point made is valid. We have changed this part to: Based on our simulations, REMO-HAM can represent the measured values reasonably.

**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?**

HAM is the aerosol module that uses the microphysical module M7. We have rephrased: ECHAM5-HAM includes the aerosol module HAM (Stier) as well as the microphysical module M7 (Vignati).

**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).**

Added as suggested: The climatology is highly absorbing, for example over Southern Europe.

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

Added as suggested: In this work, the latter has been used.

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

The chemical equations are presented in Feichter et al. (1996).

**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**

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**treated in REMO-HAM? If there is no treatment why should the model results agree with observations?**

AEROCOM emissions are widely used and unfortunately does not have higher resolution. On the other hand, we wanted to validate REMO-HAM against measurements and compare it to ECHAM5-HAM. This is why the choice was natural. More detailed emissions databases do exist and will be used in the future. For the OH concentrations, model has an artificial diurnal cycle.

**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?**

This is explained at page 755, line 24. We did do testing with BL switched on and our current research focus is on improving this part of the models.

**page 751, line 6: I cannot see that the modeled 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 Hyytiälä and Mace Head REMO-HAM even gives less good results in comparison with ECHAM5-HAM. Please explain the reasons.**

From the climate model aspect we think it is fair to say we are close. If we would look this from a process model point of view, the reviewers view would be absolutely right. For Hyytiälä and Mace Head, there are two main reasons (that we hoped to be shown by the paper): too high SO<sub>2</sub> concentrations lead to decision of not to use BL nucleation (page 752, line:6) and the spatial resolution of emissions (this explanation is added to the text): From Figure 3 we can see that the modal number concentrations between the models does not differ much at Hyytiälä. Aitken mode is smaller with REMO-HAM during the whole year, which can explain the differences in the Figure 2. One possible explanation is the resolution of emissions: based on our other simulations, ECHAM5-HAM spreads the emissions on a much wider areas than REMO-HAM. If the

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resolution of emissions is too coarse (like now coarser than the grid used in REMO-HAM), local emission sources might not be at the exactly right place. The nucleation mode concentrations are a bit higher in REMO-HAM, but the difference is not very big. The differences in Aitken mode concentrations are discussed in Section 3.1.2.

**page 754, line 24: Figure 8 shows that all model versions used in the study are overestimating the measured SO<sub>2</sub> 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.**

Very true. We are currently working on this. However, as long as the model is used in studying processes that are not too much influenced by sulfuric acid and new particle formation (such as black carbon studies), the overestimated sulfur dioxide is not a major problem.

**page 755, line 14: I am confused by the statement that the differences are due to different resolutions of the emissions. In the emission sections you explained that you always use the course emission data.**

Yes, this is confusing. We have rephrased this to: The reason for differences in Aitken mode are most probably due to resolution, which affects aerosol transport (emissions spread wider with coarse resolution).

**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.**

A very valid comment, this has been corrected.

**page 758, line 11: Please explain the resolution scaling factor in more detail.**

This means that the autoconversion parameters (which are derived from LES simulations and are resolution dependent) are scaled according REMO's resolution.

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

Corrected as suggested.

**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 1 and 2 have been merged. Area selection was based on: 1) existing domain (used in another project) and 2) it included two of the measurement locations.

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

Without these figures, validation of vertical aerosol profiles would be missing (even if they are validated against ECHAM5-HAM, not measurements).

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

These fluxes come from the chemistry part (gas phase chemistry and liquid phase chemistry).

**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.**

The reason for the higher 2m-temperature in REMO-HAM is a larger sensible heat flux due to more down-welling short-wave radiation. A reduced cloud cover in REMO-HAM due to more precipitation makes the atmosphere more transparent. The latter increases the short-wave radiation at the surface, causing higher skin and near-surface temperatures. We will analyze the feedback effects more in future publications.

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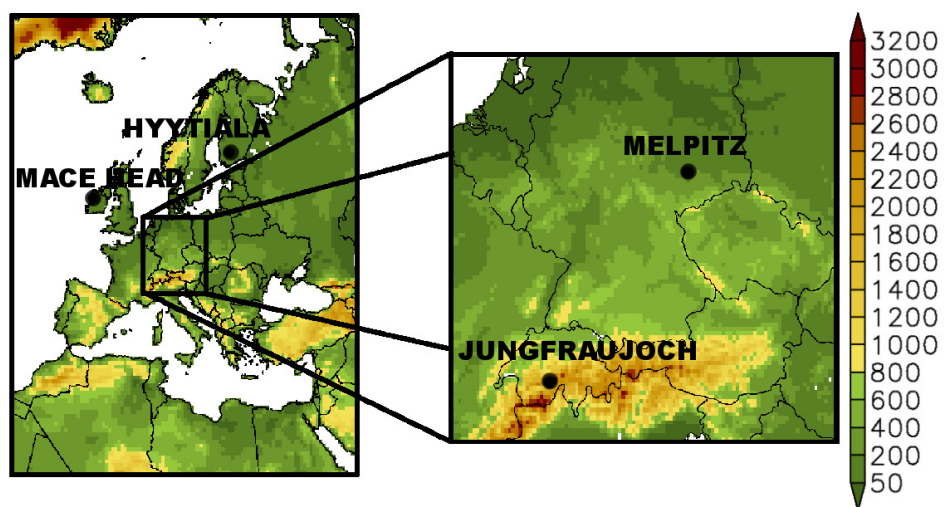


Fig. 1. Figure 1 of the manuscript

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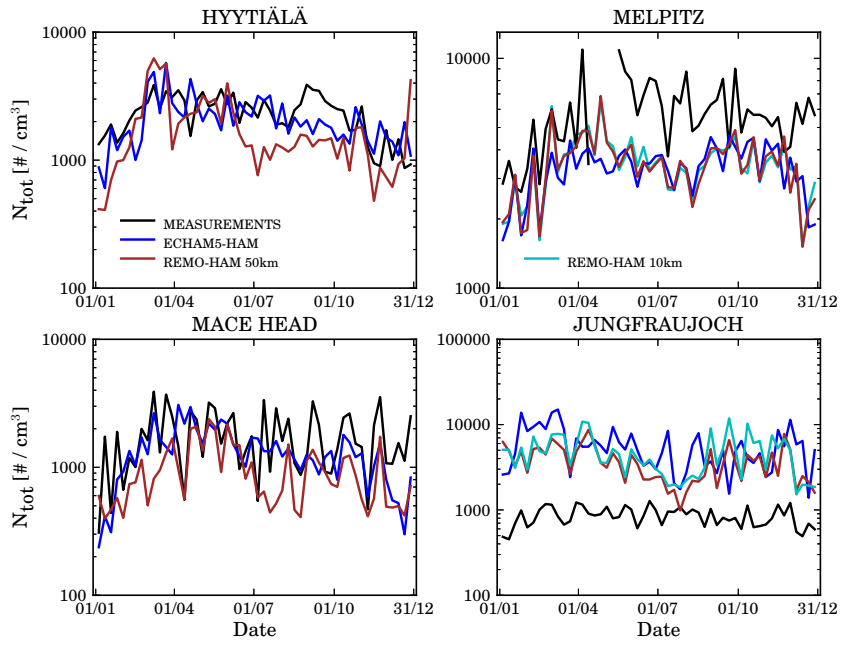


Fig. 2. Figure 2 of the manuscript

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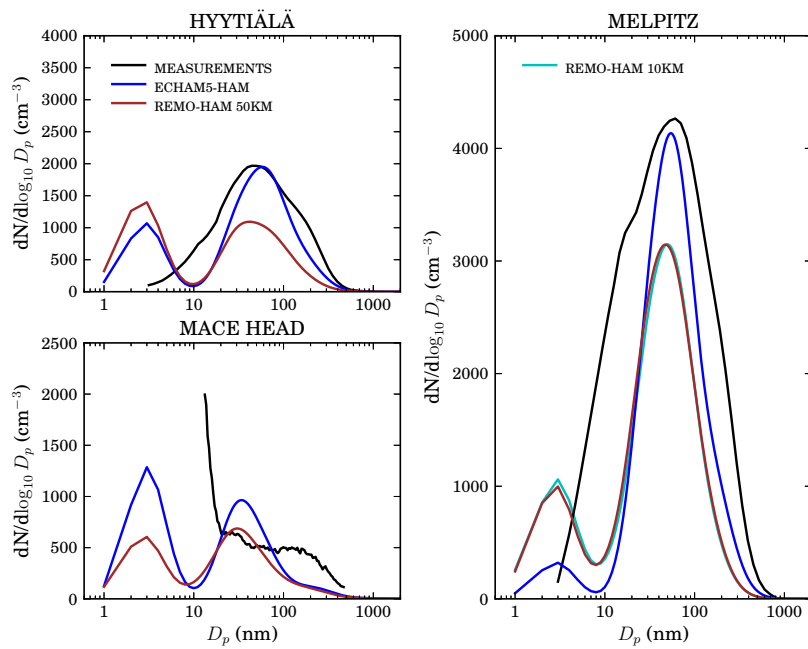


Fig. 3. Figure 6 of the manuscript

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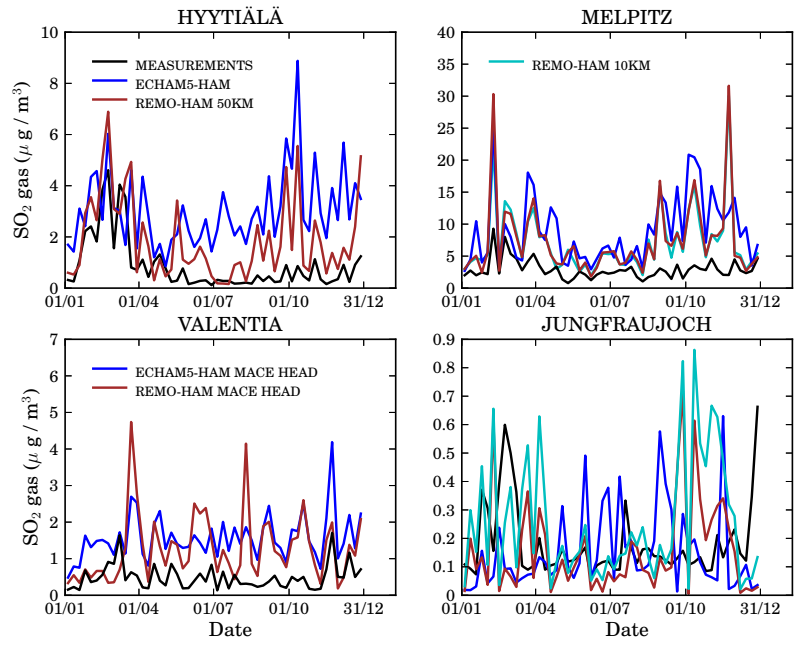


Fig. 4. Figure 7 of the manuscript