

## ***Interactive comment on “The comprehensive model system COSMO-ART – radiative impact of aerosol on the state of the atmosphere on the regional scale” by B. Vogel et al.***

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We thank the reviewer for his very detailed comments. We followed his suggestions as will be explained below.

1. The authors compare their model results with PM<sub>10</sub> measurements. PM<sub>10</sub> is often dominated by coarse particles whereas this study focuses on fine aerosol (e.g. no coarse particles considered in the calculations of aerosol optical properties). The authors might consider to compare also PM<sub>2.5</sub> data, e.g. from EMEP measurement sites where available.

We added two new figures that show a comparison of PM<sub>2.5</sub> with our simulated con-  
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centrations. Although the data shows a comparable scatter the agreement of simulated and observed values is improved. The average concentration of all data points gives almost identical measured and observed values.

2. Since this study focuses on the radiative impact of aerosol particles, I recommend including also a comparison of aerosol optical properties such as aerosol optical thickness from, for instance, satellite data or Aeronet measurements with model calculations.

We added a comparison of modeled AOD with satellite data. (Figure 9).

Specific comments 3. p. 14486, l. 16: A reference and explanation of the acronym “COSMO” should be given here instead of later (p. 14487).

We followed the reviewer’s suggestions.

4. p. 14487, l. 24: ECMWF has not been explained.

Explanation of ECMWF is added.

5. p. 14490, Eqs. (3-7): Please explain why the terms for inter-modal coagulation with coarse mode particles are missing. What are possible implications for the simulation of radiative fluxes from aerosols? Also, why is there no intra-modal coagulation of coarse particles in Eq. (8)?

Inter-modal coagulation between the coarse mode and the other modes is neglected. The number densities of the coarse mode are small. From the numbers we calculated for the individual modes we found that the inter-modal coagulation between the nucleation mode (that gives the highest number densities) and the coarse mode is two orders of magnitude less than the inter-modal coagulation of the nucleation mode particles and the accumulation mode particles. Therefore, we think that it is justified to neglect the inter-modal coagulation. Intra-modal coagulation is neglected for the coarse mode particles. Also this is justified by their low number density of the coarse mode particles.

6. p. 14493, l. 26, "[. . .] RADMKA does not take into account wet phase chemistry.": Does this mean there is no formation of SO<sub>4</sub> in cloud droplets? Oxidation of SO<sub>2</sub> by, for instance, O<sub>3</sub> or H<sub>2</sub>O<sub>2</sub> is an important pathway of SO<sub>4</sub> production. If omitted, SO<sub>4</sub> burdens might be underestimated particularly in the 'HC' case. This would have to be noted in the manuscript. Please add some comments on potential implications for your study.

At the moment SO<sub>4</sub> formation in cloud droplets is neglected. We added this in the text. As we will include wet phase chemistry within the near future we will postpone the comment on the radiative effects. As a comment would be rather speculative we will postpone this question to a follow up paper.

7. p. 14495, l. 2, Eq. (19): I guess the pre-calculated extinction coefficients are given for particles with a mass concentration of 1  $\mu\text{g}/\text{m}^3$ ?

That is correct, but unfortunately we missed a factor of 10<sup>-6</sup>. This is corrected in the final version.

8. What assumptions on chemical composition and size-distribution are made to do the "a priori calculations" (p. 14495, l. 4)? Please give more details.

The a priori calculations are based on aerosol particles that were calculated with COSMO-ART and switched off feedback between the aerosol and the radiation. For each grid-box of the model system detailed MIE calculations are carried out. The results are plotted as functions of the wet aerosol mass or in case of the single scattering albedo and the asymmetry parameter as functions of the mass fraction. Applying fit procedures we derive the parameter given in table 3. By this procedure our parameterization is based on typical size distributions and chemical compositions that are simulated in our model domain. We added these explanations to the final version of our paper.

9. p. 14495, l. 3: Why is the coarse mode not included and what does this mean for

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the calculated aerosol optical properties?

In our case the mass fraction of the coarse mode is very low, therefore the coarse mode does not contribute remarkably to the extinction. We looked for AERONET data at the station Karlsruhe (Germany) and found for both episodes (LC and HC) a small contribution (< 15%) of the coarse mode to the aerosol optical depth. This might be different in the southern part of the model domain where mineral dust contributes a lot to the total aerosol load. In that case the coarse mode becomes important for the radiative transfer. We studied the effect of this aerosol type and the results of these studies will be presented in a follow up paper.

10. p. 14495, Eqs. (21, 22): A common way to calculate average single scattering albedo and asymmetry factor is to weight single scattering albedo with its corresponding extinction coefficient and asymmetry factor with the product of its corresponding extinction coefficient and single scattering albedo. This is not done here. Why? Please give a reference or explain.

Indeed the methods we applied here (equations 21, 22) are a simplification as we calculated the effective single scattering albedo and the asymmetry parameter by weighting the values of the individual modes by their mass fraction. We will change that in future applications according to the reviewer. We carried out offline calculations to quantify the deviations of our method. They are in the order of a few percent. Only at a few grid points changes up to 20 % were found. We added a comment on that to the final version of our paper.

11. p. 14495, l. 16: Change "Table 4" to "Table 3".

We changed it.

12. p. 14497, Sect. 2.5.1: Please be more specific on how are soot emissions handled in the model. Are all soot emissions including EC<sub>2.5</sub> and EC<sub>10</sub> assigned to the "s" mode or are those emissions added to the "c" mode ("Direct PM<sub>10</sub> emissions" [Tab.

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1)]? What initial particle diameters are assumed?

This is explained in more detail in the revised version of our paper.

13. p. 14499, l. 6: Do you have a buffer zone in which the meteorological fields are nudged to the boundary conditions? If so, is this buffer zone taken into account when analyzing the results? Which variables are prescribed at the domain boundaries and how do you treat cloud liquid / cloud ice? Please give more details.

Yes we are using a buffer zone and standard COSMO treatment of the meteorological variables is applied at the boundaries.

14. p. 14500, l. 10: Change "was" to "were". We changed it.

15. p. 14500, Sect. 3.1: Do you compare wet or dry PM10? Please be more specific.

We compare dry PM10 concentrations. We added this in the text.

16. p. 14500, l. 19: Do you mean spatial correlation coefficient? If so, please say so.

In fact the correlation coefficients we calculated are not spatial ones as we included data of different days. For that reason skipped the correlation coefficients in the final version of the paper as they might be misleading.

17. p. 14501, l. 3-4, "Furthermore we prescribed clean air at the boundaries of our model domain [ . . . ]": Again, do you have a "buffer zone" that is not taken into account when analyzing the results? Otherwise this approach might result in a severe underestimation of aerosols and precursor gases close to the domain boundaries that needs to be noted in the manuscript. Please give more details.

The gaseous and the particulate species are treated at the lateral boundaries in the same way as the atmospheric variables are treated. Therefore, the outermost 5 grid points of the model domain are not included for the averaging procedures.

18. p. 14503, l. 28, "However, in areas with fewer clouds as the North Sea and the

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Netherlands negative values of \_EG and high aerosol concentrations coincide.": Can you give an average spatial correlation coefficient? 19. p. 14504, l. 7-9, "[. . . ] the correlation for episode HC [ . . . ] is small": Please give numbers. 20. p. 14504, l. 11-12, "This behaviour is a result of several nonlinear feedback mechanisms and cannot be addressed to a single process.": Which processes are most important? Please give more details.

We added a new Figure and additional explanations to clarify the three last comments.

21. p. 14504, l. 2-3, "Figure 9 shows [ . . . ] in the lowest model layer [ . . . ]": The caption of Fig. 9 says "2 m temperature". Does your "lowest model layer" really represent 2 m temperature?

No it does not. The lowest model layer represents the first 30 m of the atmosphere. The 2m temperature is derived by interpolation applying atmospheric boundary layer parameterizations. We correct that in the final version of the paper.

22. p. 14505, l. 22-25: The authors might consider to also mention that their study investigates the radiative impact of all (natural and anthropogenic) aerosols whereas the study by Bäumer and Vogel (2007) relate changes in the weekly cycle of temperature range to anthropogenic (aerosol) emissions.

We mention that in the final version of the paper.

23. p. 14506, l. 18-20, "[. . . ] and an underestimation in the order of 40% was found.": How does this translate into radiative forcing of the aerosols? Again, PM10 might be dominated by few coarse particles that are less relevant for the radiative impact of the aerosol population. An additional comparison of model results with measured aerosol optical properties such as aerosol optical thickness might help to answer this question.

We agree with the reviewer and made changes in the text according to our explanations given above.

24. p. 14515, Tab. 3: I suggest adding a reference to Eq. (20) for the 'missing' single

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scattering albedos (spectral bands 1-3, modes "ic" and "jc") to the table caption. For the spectral bands 1-3 of modes ic and jc the single scattering albedo are functions of the soot fraction according to equation 20.

We added a comment to our table heading to explain the stars in table 3.

25. p. 14522, Fig. 4: The individual subfigures and their labels are too small and should be enlarged.

We will enhance the quality of the figures for the final version of the paper.

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