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Atmospheric Chemistry and Physics Discussions

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.

Anonymous Referee #2

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General comments

The authors present the new regional model system COSMO-ART that takes into account gas phase chemistry and aerosol microphysics. The model is applied to two summer scenarios over Europe to study the impact of aerosols on radiation and temperature by conducting pairs of model simulations with and without taking into account aerosols in the radiation calculations.

The manuscript is well written and a valuable contribution to the regional modeling community. I suggest publishing in *Atmospheric Chemistry and Physics* after

addressing the minor comments and suggestions given below.

- The authors compare their model results with PM_{10} measurements. PM_{10} 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 $PM_{2.5}$ data, e.g. from EMEP measurement sites where available.
- 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.

Specific comments

- p. 14486, l. 16: A reference and explanation of the acronym ''COSMO'' should be given here instead of later (p. 14487).
- p. 14487, l. 24: ECMWF has not been explained.
- 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)?
- p. 14493, l. 26, "[. . .] RADMKA does not take into account wet phase chemistry.": Does this mean there is no formation of SO_4 in cloud droplets? Oxidation of SO_2 by, for instance, O_3 or H_2O_2 is an important pathway of SO_4 production. If omitted, SO_4 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.

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- p. 14495, l. 2, Eq. (19): I guess the pre-calculated extinction coefficients are given for particles with a mass concentration of 1 μg/m^3? What assumptions on chemical composition and size-distribution are made to do the "a priori calculations" (p. 14495, l. 4)? Please give more details.
- p. 14495, l. 3: Why is the coarse mode not included and what does this mean for the calculated aerosol optical properties?
- 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.
- p. 14495, l. 16: Change "Table 4" to "Table 3".
- p. 14497, Sect. 2.5.1: Please be more specific on how are soot emissions handled in the model. Are all soot emissions including $EC_{2.5}$ and EC_{10} assigned to the "s" mode or are those emissions added to the "c" mode ("Direct PM_{10} emissions" [Tab. 1])? What initial particle diameters are assumed?
- 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.
- p. 14500, l. 10: Change "was" to "were".
- p. 14500, Sect. 3.1: Do you compare wet or dry PM_{10}? Please be more specific.
- p. 14500, l. 19: Do you mean spatial correlation coefficient? If so, please say so.

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- 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.
- p. 14503, l. 28, ”However, in areas with fewer clouds as the North Sea and the Netherlands negative values of ΔEG and high aerosol concentrations coincide.”: Can you give an average spatial correlation coefficient?
- 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?
- p. 14504, l. 7-9, ”[...] the correlation for episode HC [...] is small”: Please give numbers.
- 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.
- 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.
- 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, PM_{10} 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.

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- p. 14515, Tab. 3: I suggest adding a reference to Eq. (20) for the ‘missing’ single scattering albedos (spectral bands 1-3, modes ‘’ic” and ’‘jc”) to the table caption.
- p. 14522, Fig. 4: The individual subfigures and their labels are too small and should be enlarged.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 14483, 2009.