

Interactive comment on “Aerosol distribution over Europe: a model evaluation study with detailed aerosol microphysics” by B. Langmann et al.

Anonymous Referee #2

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The paper describes a modelling study of aerosol distributions over Europe, calculated with the regional climate model REMOTE that includes the aerosol module M7. Aerosol sulfate is calculated explicitly. OC, BC, dust and sea salt are emitted as primary particles. The model considers also direct emission of organic aerosol matter from the ocean surface, following recent measurements.

The paper presents simulated concentration fields and compares simulated and observed concentrations for aerosol species and atmospheric trace gases. Sensitivity studies are carried out to investigate effects from European biomass burning, diurnal variability of emissions, and domestic wood burning.

The subject of the study is suitable for ACP and ACPD. The paper however lacks basic

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information on model characteristics, chemical scheme, and emissions. The figures show substantial discrepancies between simulated and observed concentrations, but the discussion is rather brief, except for POC. The model meteorology is hardly discussed. If the simulated meteorology is not accurate, e.g., in terms of BL characteristics or cloud water, then the representation of all chemical species suffers from that. Except for sea salt, the concentrations of the aerosol species appear to be underpredicted consistently, and this also is true for summer O₃. First it should be known if simulated transport patterns, vertical mixing in the troposphere etc. are representative. Maybe this was done in earlier studies, but then it would be informative to briefly summarize these findings in the paper in order to appreciate the chemistry results. Specific comments are given below. The manuscript is suitable for publication in ACP after the questions and comments have been properly dealt with.

p.17897. Please include some basic model information: horizontal and vertical resolution, time step. The paper does not discuss much on the simulation of the meteorology. What is known about this from previous studies that may be of importance to the present work?

p.17897. Are all organics taken up in the soluble aerosol only? Organics are emitted into the accumulation mode in the model, but in the measurements organics were found to make up a major fraction of the nucleation mode. How does this influence the simulated distribution? How much organic matter is emitted from the ocean compared to sea salt or DMS?

p. 17898, lines 6-8: This approach for in-cloud sulfate formation implicitly assumes that all accumulation and coarse mode particles participate in cloud formation. What is the result of this assumption? Can you exclude that smaller (Aitken/nucleation) particles may be involved in cloud formation when accumulation particle concentrations are small?

p. 17898, line 22: has been carried out

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p. 17900 and Figs 2 and 3: Wind speed appears to be underestimated at Mace Head, does this also apply to remote marine regions? Sea salt wet deposition, on the other hand, is overestimated. The text mentions that sea salt concentrations are also too high, how can that be explained in view of the underestimation of wind speed?

p.17901, line 15: Oxidant limited conditions apply to winter, not to summer. Probably other reasons play a role in the summer overestimation of SO₂. SO₂ appears somewhat smaller than observed in winter, when oxidant limitation can be expected. Sulfate concentrations appear smaller than observed throughout the year. The discrepancies deserve a more thorough discussion: e.g., what is the role of the boundary layer here, is the vertical resolution of the model sufficient for realistic mixing throughout the BL, is venting of BL air to the free troposphere realistic, what could be gained with size-dependent dry deposition which is not considered in this study?

p.17901, line 20: In my view the agreement is not that good, there is a consistent overestimation for low O₃ concentrations, especially in summer. In summer, the model appears to consistently underpredict O₃ for concentrations exceeding ~ 120 ppb. Although this may partly explain the underestimation of sulfate concentrations, this is not the most important factor. Most important oxidant for SO₂ in summer in the aqueous phase is H₂O₂. Why is this not mentioned, are H₂O₂ and associated SO₂ oxidation considered in the model? The discussion is too brief, especially when compared to the discussion regarding POC which covers more than three pages. Also, oxidation by O₃ is highly sensitive to cloud water acidity, is the simulated acidity representative, how is it calculated?

p. 17903, lines 4-5. Note that Roelofs (ACPD, 2008) presents GCM simulations of the impact of ocean organics on clouds, based on the same measurements.

p.17903, lines 11-12: are these other locations continentally located? A large influence of ocean OC is not expected here. How large are organic emissions considered in the model? I read in this particular section that SOC is not considered, are the organics

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associated with SOC emitted as primary particles or not emitted at all in this study? Please make this clear in the model description section. If 70% of the matter (p.17904, line 15) is not considered then a comparison as in Fig 7b does not make much sense. Is POC also added to insoluble aerosol mode? I expect when organics are assigned to soluble aerosol only, the OC lifetime is smaller and concentrations are smaller than when part of the OC is insoluble.

p.17904, line 11. make an important

p.17906, Fig. 9. This figure illustrates the importance of meteorology (venting, vertical mixing), expressed in nearly identical concentration profiles above 1 km for both chemical schemes. It would be interesting to see the differences also for the surface concentrations in Fig 4, large differences between both versions would point more directly to specifics of the bulk and M7 schemes. How does in-cloud sulfate formation compare between both model versions, is there a large difference?

p.17908, lines 3-4. I find this conclusion not convincing or justified if it is not clear how the simulated meteorology contributes.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 17893, 2007.

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