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Atmospheric Chemistry and Physics Discussions

Interactive comment on "PM measurement campaign HOVERT in the Greater Berlin area: model evaluation with chemically specified particulate matter observations for a one year period" *by* M. Beekmann et al.

Anonymous Referee #2

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Review of "PM measurement campaign HOVERT in the Greater Berlin area: model evaluation with chemically specified particulate matter observations for a one year period" by Beekmann et al.

General comments This paper presents a modeling study in concert with daily PM observations at a dense surface network in Berlin area for a full year period with a special focus on carbonaceous aerosols and other inorganic aerosol species. This research includes interesting and important results which are worthy of publication for air quality modeling purpose. The paper is generally well written but can be improved

significantly. I recommend that relatively minor but important revisions be made in the paper before acceptance.

Specific comments 1) Page 7296, line 21, a) “Results are similar for the Ĕ” This is very interesting because horizontal resolutions for two models differ by factors of 6-7 but their results are very close. Is this because of the use of consistent sources or due to chemical processes for SIA production? Please address this issue in view of minor comment 6). b) Does this indicate that the continental scale model with 30 km resolution is good enough for air quality modeling of SIA?

2) Section 4.2, a) Seasonal variation of sulfate is quite interesting because it does not show summer peaks due to higher oxidant concentrations which are observed in the United States. This seasonal contrast was previously addressed in Kasibhatla et al. [1997]. This feature is well captured by the model and needs to be explained more. b) Minimum PM10 concentration in summer is well reproduced by the model. Please discuss the reason for this. c) I wonder if there is an influence of biomass burning on OC and EC concentrations and their seasonality. d) Authors may want to look at sea-salt sulfate contribution in winter when large underestimation of sulfate in the model occurs. e) Figure 3, Please provide the information for the different color bars.

3) Section 4.3, a) Correlation coefficient is sensitive to highest values. Does this mean that highest values captured by the model in different seasons are mostly due to the transport resulting in higher correlation? I highly doubt about that. b) Underestimation of EC and OC at rural sites can be related to its wet scavenging (too much loss). Please address this issue in view of minor comment 8).

4) Section 5.2, Is there CO observation available? If it is, please discuss the correlation between CO and EC and the possibility of its use for source scaling.

Minor comments (technical corrections) 1) Page 7286, Line 22, I guess that “knowledge” is an uncountable noun. In the following sentences, authors may consider rewriting them in a clearer way such that individual aerosols “are produced from” their pre-

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cursor gases rather than they “are related to” gaseous species.

2) Page 7288, Line 1, “large” would be better than “strong”. This sentence may read misleading because EC and OC aerosols are generally fine. In other words, underestimate of EC and OC could be due to missing sources rather than their fractional dependence on size.

3) Page 7290, Line 18, SAPRC-93 must be a typo and should be SAPRC-99. Please cite a relevant paper.

4) Page 7291, Line 12, If the mixing height is higher than the model top (3 km), the model has to use only three layers (one with 20m thick and other two with approximately 1.5 km thick) for the simulation. Is that true? Were there any occasions for this to happen in a year-around simulation in this paper?

5) Section 2.3, It is not clear whether or not the meteorological data used in CTM are from the model or are based on the observations. Please clarify it in this section.

6) Section 2.4, Actual numbers for individual species emissions in the model would be valuable for readers who are interested in PM modeling. Please provide them in the text or in table. Emissions for continental scale model and for nested model seem different but their magnitudes were scaled in order to make both consistent. Is this conducive to the consistent simulations between two models discussed in section 4.1? Are there any differences in spatial distribution between two emission sets?

7) Section 2.6, It is not clear whether or not the model includes anthropogenic VOC for the secondary organic aerosol. Please clarify this. Can this be related to the OC bias in section 4.1 especially at traffic sites where OC low bias is larger than that of EC?

8) Section 2.7, It is necessary to explain how the model simulates the wet scavenging for EC aerosol because EC is initially hydrophobic but becomes hydrophilic by oxidation and by coating with other soluble aerosols. Therefore most of models treat these two types of EC tracers: one is soluble and wet scavenged, other is not. The same goes

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for OC.

9) Page 7304, Line 24, Authors describe “internal mixing” which should replace “external mixing” in that sentence.

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 7285, 2006.