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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Response of fine particulate matter concentrations to changes of emissions and temperature in Europe" *by* A. G. Megaritis et al.

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1. *This was a useful and interesting paper, particularly from the standpoint of science policy: the authors make use of a regional air pollution model to carry out multiple scenario simulations over Europe, investigating the possible effects of changes in different primary emissions on ambient concentrations of particulate matter. My concerns with the paper are more with presentation and formatting rather than the methodology - I've therefore rated these revisions as minor, though some rewriting of the paper would help make it more accessible to the scientific and policy community. The paper will be suitable for publication in ACP after the following revisions: Section 4 (Results) needs*

to be condensed and reorganized. The information presented is useful, but the length makes it difficult for the reader to form an overall impression of what has been learned through the authors' work. This also makes it harder to assess the relative impact of the different scenarios on the particulate matter generated by the model.

We have done our best to improve the presentation of the material without any changes to the conclusions. Section 4 has been rewritten reducing its length and focusing on the main insights gained from the simulations.

2. *Table 2 is invaluable in this regard: I would strongly recommend that the authors create a larger summary table of their results, to be presented at the start of section 4, with a breakdown by regions I-V, with values and percentages for each region. Subsequent discussion should be with reference to this table as well as to the figures in the submitted manuscript. One axis of the table (e.g. columns) should contain the different scenarios, while the other axis (rows) should contain the impact by either region (regions according to the authors' Figure 1), or by country, and by species. The cells of the table would contain the average change and their percentages, for each region. This table will allow an easy, quantitative comparison of the results of the different scenarios, and would make the work much more accessible to the scientific and policy communities who would be interested in referencing the authors' results. This should also result in a shortening of the text of section 4, which is currently too long and in need of summarizing. An example format for such a table is attached to this review as a bitmap (see Fig 1).*

Following the reviewer's recommendation we have extended Table 2 presenting our results from the emission reduction scenarios, based on the impact that they have on PM2.5 components in different areas over Europe. The new summary table gives the average concentration change and the corresponding percent average change of each PM2.5 component resulting from the different emission reductions scenarios, for each of the regions I-V of the domain (according to Fig. 1). This now includes part of the information that was originally included in Section 4.

3. *The section on model evaluation is extremely brief; here, more details are required. The impression given is that the authors made use of only 4 stations for their model evaluation, which seems inadequate for the evaluation of a regional model code (compare to multi-model comparisons by McKeen et al; TEXAS-AQ papers, or AQMEII comparisons, where multiple stations over a long time period were used). More stations should be used, or an argument as to why this could not be done needs to be presented. The authors state their evaluation in terms of percent agreement within given ranges – they should also state other standard statistical measures, such as the mean bias of the model with respect to the observations, the mean error, the root mean square error, etc.*

The detailed model evaluation has been presented in other work (Fountoukis et al., 2011). In this study, we compared the model predictions against hourly average ground measurements, taken at four European measurements stations, as well as airborne measurements from aircraft flying over Europe. The evaluation was based on approximately 8500 measurements and examined the performance of the model on an hourly basis over an extended area. The area covered (thanks to the airborne measurements) and the number of data points for the various PM components make this one of the most extensive evaluations of a CTM over Europe. Our evaluation focused on the AMS measurements and not on routine PM measurements because of the issues involved in the interpretation of the latter. These issues involve problems with the significant amounts of water that is often involved in the PM_{10} measurements in Europe and the significant contribution of the difficult to simulate dust in these concentrations. The model used in this study has recently been evaluated in the Eastern US (Karydis et al., 2007; Murphy and Pandis, 2009) and Mexico City (Karydis et al., 2011; Tsimpidi et al., 2010; Tsimpidi et al., 2011).

We do understand the point of the reviewer and we have added a summary of the

evaluation results in the main paper and some additional summary evaluation statistics in the Supplementary Information.

Minor issues:

4. *The time period over which the simulations took place is not given – how many summer days were simulated? How many winter days?*

We simulated a month for each period. The time periods over which the simulations took place are now given in the revised manuscript.

5. *The authors should make an argument as to whether or not their time period is representative of typical conditions for the region studied.*

Hamburger et al. (2011) have provided an extended analysis of the synoptic and pollution situation over Europe during the May period simulated here. Summarizing both periods included a variety of meteorological conditions and pollution levels. For example, the first half of May was characterized by a blocking anticyclone leading to stable meteorological conditions and high pollution levels over Central Europe. The rest of the May period and the winter period were relatively typical. A summary of the meteorological conditions and references to the corresponding EUCAARI papers have been added to the revised manuscript.

6. *Suggestion on the colour contour maps: could the same scale be used on each panel for a given sensitivity run figure? This would allow the reader to easily distinguish the relative impact on the different components of PM.*

We have redrawn the figures using the same scale on each panel for all the sensitivity run figures.

7. *Figures 3 and up show reductions in PM2.5: add "(control-scenario)" to the figure* *captions to indicate that red areas represent reductions, if that's the intent.*

We have changed the figure captions following the suggestion of the reviewer to avoid any potential confusion.

8. *VOC:NOx discussion is quite interesting (page 8784): the discussion might be aided by a plot of the average VOC:NOx ratio (including the 5.5:1 value as a contour line), to show how the ratio changes with the VOC and NOx scenarios.*

We have followed the reviewer's suggestion and included a figure presenting the predicted base case VOC/NOx ratio during the summer period, showing with red colour the areas where VOC/NOx ratio is higher than 5.5:1 (NOx-limited areas), and with blue, areas where VOC/NOx is lower than 5.5:1 (NOx-saturated areas). In the same figure we have also included the predicted VOC/NOx ratio after the reduction of NOx and anthropogenic VOCs emissions, during summertime.

9. *Page 8791, first few lines: one important conclusion here is that the nitrate increases are less than the sulphate decreases; the net impact on fine PM is a reduction. Similar increases in nitrate have been noted in North America following reductions in the emissions of sulphur dioxide in the Ohio River valley.*

We have stressed in the revised manuscript this important point. Even if theoretically the reduction of sulfate can be balanced by the increase of nitrate leading to no change of fine PM or even an increase in extreme circumstances, in this case the net average impact is a reduction of fine PM.

10. *Page 8793, line 1: begs the question: how well does the model do in comparison to obs for the relative ratio of SOA to POA? AMS measurements were mentioned under model evaluation section: was there an opportunity to compare HOA to OOA ratios, for example?*

In general the model agrees well with the PMF analyzed AMS data. For example, compared to a PMF analysis of the organic aerosol AMS data at Finokalia during the EUCAARI May 2008 campaign, the model correctly predicts negligible HOA concentrations at Finokalia as a result of no strong local sources and rapid conversion of POA to OOA. In addition both AMS and the model give high oxygenated organic aerosol concentrations (more than 98 percent of total OA, on average). We have performed similar comparisons with the data in the other measurements locations. Both measurements and model predictions (see Fountoukis et al., 2011) are consistent suggesting that most of the OA is oxygenated. The situation changes a little during the winter with higher levels of HOA but with the OOA still dominating in the areas away from the urban areas. A summary of these results has been added to the revised paper. A detailed comparison of the results of the PMF analysis of the AMS data and the PMCAMx predictions is in progress and will be published in the near future.

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