

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

**Anonymous Referee #2**

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Review of Megaritis et al. entitled “Response of fine particulate matter concentrations to changes of emissions and temperature in Europe”.

Main comments The paper presents the results of a series of sensitivity simulations with the PM-CAMx model. The information provided in the paper is of interest to the community and especially for those that work on emission control strategies. However, I feel that the paper can be largely improved through an improved model evaluation, an emphasis on the broader picture in stead of all the separate runs and a discussion of the results e.g. in comparison to others.

The evaluation for PM-CAMx is extensively performed for the US. However, for Europe

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hardly any evaluation is available. As climatological regimes and emission regimes are quite different from the US and input data are differently defined in European practices a thorough evaluation seems necessary, prior to using the model for detailed sensitivity studies. The mentioned AMS campaigns cover only a few weeks in total and do not cover all PM components. Moreover, they do not provide a good coverage across Europe. I wonder why a comparison to chemical data from the EMEP network and PM data from AIRBASE is not presented for the period at hand. Inclusion of a standard statistical is therefore advised.

Chapter 4 is very lengthy. For each simulation the results are discussed per component. This is also shown in the conclusion section. I would favour a more condensed presentation of the results and an interpretation of the comparison of the different control options and the fact that these may have different impacts and order of effectiveness in different parts of Europe. The study only shows control strategies for single components. It should at least be shortly discussed which combinations of pollutant reductions might be promising considering the outcome of this study and the non-linearity's that may occur. Many absolute and relative changes are mentioned. These may not show the same patterns and maybe should be separated a bit more.

A discussion of the results is missing. Now and then other studies are named but their outcome is not given and is not set in relation to the outcome of this study. No shortcomings of this study are discussed, nor are recommendations given. Is the coupling of the different components in the atmosphere 100% represented by the model? Are all needed components modelled? What are the most uncertain parts of our knowledge and the model description? Which results are robust, which are less certain, which are contradicting other studies? The selected periods are not typical periods for winter and summer conditions in Europe. I think both may also be characterised as spring. A discussion on the representativity of the simulations for a longer periods appears to be needed.

All remarks listed below refer to the PDF of the word document

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#### Detailed comments

General: Please write out all abbreviations at first usage (OA, POA, VOC, etc).

Abstract, last sentence: Please rephrase as temperature is not a controlled variable.

Introduction, page 3, line 3-24: The text on the impact of climate change on air quality focusses on ozone completely, whereas pm is under emphasis in the paper. There is information on particulate matter too. Could you incorporate this too and maybe shorten the ozone discussion a bit?

Introduction, page 4, line 29: here it is claimed that PMCAMx can accurately and efficiently describe the physical and chemical transformations of gas and aerosol species. This sounds very confident as many challenges are left in the modelling of PM and precursor species. Moreover, when very efficiently why not model a 1-5 year period? Other models can. So please rephrase or remove this part of the sentence.

Section 2.1: please also mention the emission modules for sea salt and dust. Please include removal mechanisms. Does the dry deposition scheme include temperature dependent stomatal conductance description?

Section 2.2: Are the WRF data for Europe evaluated? Please add reference or summarise results. The GEMS emission dataset is quite old and was replaced by the MACC dataset. Why was this set not used? How do you treat emissions as function of time, height, VOC split, etc Please provide a reference for the EC-OC emission database. You use the VBS approach. Do you assume additional (condensable) emissions that are not inventoried in the emission database? If so, please specify. Please specify the boundary conditions used.

Section 2.3: This evaluation is too limited to be able to understand the model performance and shortcomings that are important to interpret the results (see above).

Section 3: please explain the reason for the maxima above sea. The distributions for winter and summer are atypical for some species (e.g. nitrate – no broad distributions

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across continental EU in winter, no Po-valley maximum), OA – Germany minimum, PM2.5- lack of high concentrations over south eastern Europe) which may be due to the special events occurring in these periods. Some words on the representativity of the conditions in EU during the periods is needed I think. March 2009 was summerly hot for instance in western Europe.

Section 4.2: Ozone increase in western Europe in case of NOx reduction is presumably due to less titration in this VOC limited area. The increase of ozone in urban centres is well known and has been shown many times, and is certainly not new.

Chapter 5. First paragraph: so temperature is increased everywhere. Does this also include the dry deposition and additional stomatal closure at high temperatures? Page 19, line 25: Paris is mentioned here for POA impact. Considering the MEGAPOLI campaign results, is this a realistic result? How does it translate to other Urban signals in the pictures?

Conclusion section: Final paragraph is very weak. It is mentioned that Control strategies may play a crucial role!!!! This key. Please rephrase.

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Interactive comment on Atmos. Chem. Phys. Discuss., 12, 8771, 2012.

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