Geoscientific
Model Development
Discussions

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Interactive comment on "Dynamic model evaluation for secondary inorganic aerosol and its precursors over Europe between 1990 and 2009" *by* S. Banzhaf et al.

Anonymous Referee #2

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The authors present a detailed and comprehensive evaluation of a 20 year simulation with the LOTOS-EUROS model. The focus of the paper is on secondary inorganic aerosols and precursors across Europe with special attention on the long term trends. The simulations are based on TNO MACC emissions and metrological fields from the RACMO2 data for the full period, which secures a consistent setup important in trend studies. The paper also includes an interesting discussion of non-linear responses to emissions changes that makes it highly relevant for the CMT community. I therefor recommend that it is accepted for publication in GMD, however there are some issues that needs to be analyzed and discussed in the paper before it can be accepted.

General comments:

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The first reviewer has two main comments to the paper regarding the statistical significance of trends and the three different time periods used and the possible impacts on the results. I agree with the first reviewer that especially the impact of change in numbers and locations of sites needs to be addressed before the final version of the paper is published.

I also wonder how it affects the overall conclusions that some of the trend analyses are based on data from e.g. mainly Northern Europe. Shouldn't you then compare it to the trend of the emissions in this region? Even if most of the components have a long life time, the emissions in the specific region will have a significant impact on the level of SIA within the region.

The spatial distribution of the MACC emissions are as far as I understand kept constant (based on the distribution for 2005). It is mentioned only briefly that this could impact the results. However, in order to give a more qualified estimate on how this will influence the simulated trends it would be usefull to include more details on this. This could e.g. be done by comparing the MACC emissions with the EMEP emissions for 1990 (assuming that the EMEP emissions include spatial changes …)? This has perhaps already been done during the evaluation of the MACC emissions and a reference to this work and a few lines on the conclusion could be sufficient for the paper.

On page 4654 it is mentioned that trends are not included in the boundary conditions. Please elaborate a bit on the possible consequences of this in the in the discussion. Maybe other studies have investigated this in more detail? Please also mention where the used background concentrations are taken from.

Specific comments:

P. 4652 line ca 7. It seems like the models has relatively few vertical layers. Please give an indication on the approximate height of the layers.

The source apportionment module needs to be described in more detail.

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Technical corrections:

P. 4652 line 19. Delete "()" around "Erisman et al."

There seems to be a general lack of "commas" in the text. Pleas review the manuscript with this in mind.

P. 4662, line 10. Make it more clear in the text that Fig 2b is for German stations only, not all of Europe.

P. 4665, line 12. "Other than for . . ." – reformulate, bad English language.

Fig. 4 – in the current version of the manuscript it is very hard to see the different lines in this plot.

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