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Geoscientific Model Development Discussions

Interactive comment on "High resolution air quality simulation over Europe with the chemistry transport model CHIMERE" *by* E. Terrenoire et al.

Anonymous Referee #1

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Chemistry transport models play a key role in the definition emission control strategies. It is therefore of great importance to have well validated models that operate at the appropriate scale for its purpose. In air quality modeling a tendency to operate models at increasing resolution is observed. The study at hand describes the results of a CHIMERE model simulation at 7 Km resolution across Europe. Although validation studies are very useful and deserve publication, I have a number of concerns that needs to be addressed before this paper is acceptable for publication.

First, the paper lacks a firm discussion of the results. Moreover, at many locations the explanation of the model behavior could be formulated more concise. I would recommend to include a separate discussion section in which the (at least) the following topics are discussed: a) Is the model performance of PM as robust as suggested? b)

Shortcomings in SIA modelling c) Modelling of dust d) Modelling of (S)OA e) Choice of emission data, shortcomings, impact of downscaling approach f) impact of measurement location on performance assessment g) Does the increase of resolution provide a better performance? h) Do the model developments from this study improve the performance? i) Impact of urban meteorology correction (move from method section) j) Are there other studies that indicate that NOx emissions in Europe are underestimated? Can there be other explanations for the NO2 underestimation?

In this way the three main areas for further improvement identified in the conclusions can be motivated (and maybe extended?). The topics are discussed in more detail below.

Second, the paper shows that the CHIMERE model performs quite well for total particulate mass, whereas obvious shortcomings in the modeling of secondary inorganic aerosols are shown. Moreover, I would suggest to provide and discuss the modeled distributions for dust, sea salt and (secondary) organic aerosol. These components explain a large part of modeled mass but model parameterizations or source terms are very uncertain. I wonder if the statements on the model performance for PM should not be weakened considering these issues. Also, I would advise to put chapter 4 before chapter 3 to present the distributions before the validation.

Third, the introduction is quite sparse. I think the research questions can be better motivated. Moreover, the citation of other studies (excluding those of the CHIMERE team) throughout the paper is quite thin.

Finally, the emission gridding performed in this study is not really new. Please explain why the MACC data were not used as input to this study. This database was available and avoids some of the problems that are now included in the runs here.

Detailed remarks:

Introduction: Please introduce why CTMs are going to higher resolutions. Do other

studies see improvements in performance when increasing resolution?

P4139, line 4: the Collette paper deals with regional trends P4139, Line 5: GEMS = GMES P4139, line 12. Please mention the comparison studies themselves.

Model description: Please specify the version number, methodology for wet and dry deposition, vertical extend, and upper boundary conditions.

P4140, line 13: the wording suggest that you have a nest in a larger CHIMERE simulation? True?

P4140, line 18: the specified domain does not cover the whole Europe.

2.2 Meteorology: First paragraph: why is the WRF and ECMWF data compared in this paper? It was not one of the model developments listed in the conclusions. Maybe a textual description is enough.

P4142. Line 8-19: here a description of results is given. Maybe this could be moved to the discussion section?

P4143, line 11: SNAP 11 also includes sinks? Isn't snap 11 normally used for natural emissions?

P4143: The spatial regridding is performed keeping the 0.5x0.5 degree emission total fixed? This would mean that also uncertainties present in the EMEP gridding are introduced in the high resolution emissions used here. Does this mean that all anthropogenic emissions (except agriculture) is put on urban areas? So not on road segments, rivers, canals, industrial locations, etc?

P4144: line 1. Do I understand correctly that population is not used? Or? Line 9. Please state more clearly if you used the function representative for France to distribute SNAP2 emissions outside the large urban areas in all countries? I am confused by the text. Please discuss how representative the function may be. . .

If you use the function, what happens when you keep the total of the EMEP cell fixed?

Wouldn't the Paris SNAP2 emissions return back to Paris as there is no other city on that 0.5x0.5 box? Shouldn't you redistribute the SNAP2 emissions from the country total and allow emissions to shift across the country? Or did you do that?

From the discussion on figure 5 I get the feeling that all SNAP2 emissions are going into the urban areas with the largest population density. With the weighing of the data with the wood combustion function I would assume Paris would be gone, but it is not. . . The SNAP2 emission distribution in Poland, with strong regional differences, may deserve an explanation.

P4145, line 6: lowest layer is 20m.

P4146. Line 15-25: is this explanation really completely correct? I would guess that a colder climate would have a lower seasonal variability in the temporal profiles by definition. A warmer climate has fewer days that count in the heating degree day sum, so fewer days require heating. Thus smaller emissions but with a higher variability in the temporal multiplication factor (as it is normalized to 1).

P4151, Line 11 :Somewhere the challenges for the coarse mode need to be discussed more thoroughly. Please explain why the PM10 goes less well than PM2.5 (a part of PM10). Is this because the dust and sea salt are not captured well, also in terms of timing? Other studies have shown that desert dust and sea salt events can be modeled in time quite well.

P4152, line 4: How is the cloud chemistry described?

P4152, Line 17: This statement can be stronger by looking at the background at Valencia stations. Is it the low values in the time series that are overestimated?

P5152, Line 23: Nitrogen dioxide should be ammonia.

P5153, Line 15-23: What are the typical levels of coarse nitrate in Europe? Are those levels sufficient to close the gap between observed and modeled nitrate?

Line 26: is total nitrate really much better than nitrate?

P5154, Line 1-2: The underestimation of NO2 is much less than the underestimation of (total) nitrate. The implication to the NOx emissions may be too easy, as the underestimation of NO2 is not really discussed in the paper. Moreover, the NO2 underestimation reported here is not in line with the TFMM model comparison results provided in a recent EMEP-report. The nitrate underestimation seems to be present in that study, so I wonder if the NOx underestimate is not due to other reasons. Please go into some more depth on the NO2, NO3 underestimation.

Line 6: It is very likely that the overestimation is related to SO4, as nitrate is underestimated.

P4155, line 22. Ozone also peaks above the Mediterranean because of a lack of ozone dry deposition above sea.

P4156, Line 7: Looking at the distributions of the PM stations and the underestimation it seems that the areas in southwestern and southeastern Europe are largely underestimated. Is the comparison at the monitoring stations and the interpretation of those representative for the whole of Europe?

Line 12: the sentence on dust is important and the role of dust and possible improvements should be discussed.

Line 21: Strong power sector contributions are found there too.

P4157: Could you also show dust and Organics as a distribution?

Conclusions section: I would have been interested to see the impact and degree of improvement by the mentioned developments.

P4158, line 5: here an implicit comparison to coarser resolutions is made. It says that increasing resolution provides a better temporal correlation. This is not shown in the paper, but would be interesting. Did you compare the performance to earlier runs and

the previous model version? Can you say something on that?

The points for further research are not nicely motivated. Why these three? A discussion is needed.

Line 25. The last point is a strange sentence. How can you introduce the French database, which is probably also top-down, for eastern European cities? Please rephrase. Note that this work has been done for some areas already, which could be mentioned. One of those is MEGAPOLI, in which the French teams were very much involved.

Figure 3. Step 2 should be daily disaggregation's as hourly is done in step 4. In step 4 the degree day approach is listed, whereas this provides a daily multiplication factor, not hour of the day. In step 2 it should be profiles instead of profils.

Figure 13: NO2 pictures for summer and winter seem to be reversed. Same for PM10 in Figure 14

Figure 16. The ammonium distribution for the winter shows the nitrate pattern in eastern Europe, whereas I expected to see that of sulfate. Why does ammonium concentrations minimize in the SO2, hotspots? I guess the ammonium and (2xSulphate + nitrate) are in agreement with each other, or? Please explain!

Interactive comment on Geosci. Model Dev. Discuss., 6, 4137, 2013.