

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

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General update

In the submitted version of the paper, the model resolution is referred as 7 km. This estimate was based on the geographical degree coordinates of the grid cells size ($0.125^\circ \times 0.0625^\circ$) which are actually closer to 8 km at the centre of the domain. This new estimate is now used throughout the revised version of the paper.

First, we would like to thanks the Referee #2 for its constructive remarks and suggestions. Here are the different answers regarding the Referee #2 comments.

General comments

C2899

The current manuscript describes the results of a CHIMERE model simulation at (0.125×0.0625 horizontal resolution) performed over Europe for the year 2009. The model evaluation is performed using both rural and urban background stations. Using seasonal and yearly mean statistical indicators authors attempt to highlight advantages and shortcomings of the model. The paper is poorly written and hard to follow. Authors have a complete disregard to acronym definition to the level that at times it is impossible to understand the text. What does UB stand in the abstract? It is only defined on pg. 4147. What does GFS/WRF stand for? And the list goes on and on. It is not a reviewer's job to correct all the missing acronyms. Just a point, the “Airbase” is not defined and there are three different versions “Airbase”, “AIRBASE”, and “AirBase” found in the manuscript.

Following the remark of the Referee, acronyms have been checked carefully and each acronym is now correctly defined in the revised version of the paper.

Introduction is very shallow and needs to be extended to cover other studies in the region, their advantages and shortcomings, etc. Based in this information then authors should identify the research questions and shows the clear need for the additional model development/validation.

The introduction has been rewritten to better address the research questions and more references to cover other studies in the region have been added.

I believe Anonymous Referee #1 gave a very nice review and lots of helpful suggestions. In addition to his/her recommendations, I would propose considerable restructuring of the manuscript. In the methods section, please describe the model (with clearly identified improvements) and observational data. In the results section please show the advantages of using new modules.

As suggested by the Referee, the paper are been restructured. The method section includes a brief description of the CHIMERE version used for the study, the meteorology data used (including the urban correction), the methodology for the preparation of the

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anthropogenic emissions (including the SNAP2 temporal modulation), the observation data used and a description of the data analysis methodology.

Also instead of using phrases like “logically seen”, “catches nicely”, etc. describe how does model perform for the selected species. If the agreement is poor, please explain why. Please note that by better discussion of the results I do not mean addition of new figures/Tables. There are excessive number of tables and figures in the manuscript. What is missing is clear and concise description of the results. Figures 8,9,10 as well as several tables can be removed (or put as supplementary online material).

In order to make it clearer we address the main results in a more concise way. The description of model performance is done by species and if the agreement is poor, we address explanations for this behaviour. As suggested by the Referee, a number of figures and tables have been removed or put as supplementary online material.

Specific Comments

Please clarify that CHIMERE is not a CTM but a regional CTM. The difference between CTMs and GCMs can be found elsewhere.

CHIMERE is now defined as a regional CTM in the revised version of the paper.

It is also not clear why authors believe that CTMs were initially designed only for the ozone concentration simulations.

We replaced “ozone” by “gas phase”.

Pg 4140 Ln.17 Please explain what does nested mean, nested how?

The domain used for the evaluation is a nest domain. However as the boundaries of the mother domain are close to the nested domain, its boundary conditions are strongly influenced by the climatology dataset used at the boundary of the mother domain.

Pg. 4140. Ln. 20. Fig 7 should come after Fig. 1-6.

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Figure 7 is referenced as Figure 1 in the revised version of the paper.

Ln. 4141 Ln.5. I am not sure I understand the logic here: are reanalysis data compared to the model results? If so, please explain why. If not, please reword to make it less confusing. The rational for the choice of January, 2009 as a “selected period” is also not clear.

This section was modified. The motivations for the meteorological driver are now textual and we added some more references.

Pg. 4141, Ln. 25. Please explain how the model with 7 x7 km² horizontal resolution and 8 vertical layers can resolve the urban canopy. How were wind speed and the vertical diffusion (Kz coefficient) modified within the CHIMERE version used in the current study to resolve the urban boundary layer? What else was “corrected”? What is meant “rather strong impact”? What does qualify as strong vs. weak? Instead of comparing model-to-model, Figure 2 should compare model results (with and without urban canopy correction) to measurements. This chapter should be made as part of the results section and should be expanded.

In order to integrate the influence of the urban canopy on meteorology, the wind speed and the vertical diffusion (dispersion coefficient, Kz) are modified within the CHIMERE version used for this study. The description of the modification is available in the meteorological section and quantify in the discussion section using 2D concentration maps and time-series as suggested by Referee.

Pg. 4142, Ln 22 Change “Non Methanic” to “non-methane”

The correction has been done.

Pg. 4143, Classification should either go into a table or be removed.

As the classification is well known it has been removed from the revised version of the paper.

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Pg. 4143, Ln. 20 Please reword “whose high resolution (1 km) preserves the accuracy in term of emission spatialisation.”

This section was rewritten.

Pg. 4144, Ln. 15 Please include a reference for “because biomass burning emissions are less influent on gas phase pollutant than particulate matter.”

The emission inventory used is the spatialised emission inventory (INS) (Ministère de l'Ecologie et du développement Durable, 2004). The reference of this emission data inventory is used for this statement.

Pg. 4145, Ln. 11. Please explain who is IIASA and how/based on what were coefficients recommended for different chemical species. Are these just tuning coefficients for the results to be closer to the observations? If so, please state clearly. Please place the major urban cities on all figures where it is proposed that urban corrections can have considerable effect on the model performance.

The annual NO_x emissions were speciated into NO, NO₂ and HNO₂ using the coefficients recommended by the International Institute for Applied Systems Analysis (IIASA) (Z. Klimont, pers. Comm.)

Pg. 4145, Ln. 13 please reword the sentence “In a second time, an aggregation step is performed to lump NMVOCs into model species following Middleton et al. (1990).”

The sentence is now written as: “In a second time, real NMVOCs species are aggregated and associated to a corresponding model species following Middleton et al. (1990)”

Pg. 4146, Ln. 5. Please explain calculations for daily energy demand. Does the fact that the constant offset, C is used mean that production of hot water is associated with the same energy consumption during summer as for the winter?

Yes, it was a constant factor. We agree that is probably not totally correct, but as a first

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guess we considered it a reasonable approach to include and the additional energy consumption due to hot water, also trying to replicate the monthly profile generally used in modelling application over Europe (Society et al., 1994).

Also how reasonable is it to assume that energy consumption by the population is the same in developing and well developed countries, and is only function of the ambient temperature?

Degree day is a typical indicator used to compute energy demanding and designing heating plants. Being defined on a daily basis, It is also frequently used as a surrogate to modulate the temporal evolution of energy consumption for heating and, as a consequence, to modulate emissions. It is important to recall that we did not use degree days concept to estimate the integrated energy demand over the year and the consequent annual emissions, because this information was already available in the EMEP inventory as tons/cell/year. Our goal was just to modulate the provided total emission on daily and hourly basis. Thus, we used the time series of degree days computed and normalized at each cell, just as a modulation factor not as an emission factor.

Pg. 4147, Ln. 19. Please explain what does it mean “In this paper, we perform an “operational evaluation” (Dennis et al., 2010).”

The operational evaluation is now defined as: “The evaluation techniques include statistical and graphical analyses in order to determine the degree of agreement between the model and the observations in an overall sense.”

Pg. 4149, Ln. 21 Please explain what does it mean “concentrations is very well simulated. ” Are over 10 ppb differences (i.e. _40% error) in daily mean ozone concentrations acceptable?

Looking at the correlation coefficient (0.60-0.70), we confirm that CHIMERE reproduced correctly the daily temporal variation at both RB and US stations. Moreover the annual and seasonal overestimations are lower than 30 % of the observations which is

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according to the author compared to other CTM modelling studies.

Table 1 is confusing. Please explain what do S1 to S11 stand for.

Notation for SNAP sector in Table 1 has been change to avoid confusion. S1 has been replaced by SNAP1 and the change has been applied to the other SNAP.

Fig. 11. Please convert NO₂ concentrations from ug/m³ to ppbs to be consistent with the rest of the manuscript. Also explain what the different symbols are on the figure.

For more clarity, Fig 11 has been removed from the revised version of the paper and only a textual description of the results is proposed.

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