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Comment

## ***Interactive comment on “Frontiers in air quality modelling” by A. Colette et al.***

**A. Colette et al.**

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Authors: We are grateful to the reviewer for a substantive and helpful review. We addressed all the comments raised in the forthcoming revised manuscript.

### GENERAL COMMENTS

Reviewer #2: In general the paper is clearly structured but several important issues are only briefly mentioned and not explained in enough detail. It lacks for example a detailed description of the model set-up and the set-up of the high resolution emission database. One concern about the presented work is the use of meteorological input fields with a comparable coarse resolution of 16km. The impact of the low resolution meteorological input data and how this data is interpolated on the 2x2km<sup>2</sup> grid should be discussed in more detail as this is an important issue as it limits the possible improvement of the simulations.

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Authors: The section devoted to the method has been very substantially revised to provide more details on the model setup, the emission downscaling methodology, and the possible impact of using 16km meteorology to drive the CTM simulations.

Reviewer #2: It is difficult to comprehend the motivation of this study beside the technical aspect, therefore a more elaborated motivation and discussion of the additional value of high resolution simulation especially over rural regions and on the continental scale would be good. In this context the study should also be better embedded in the current research related to this topic by giving more references to relevant studies. The presentation of the results is rather short and mostly limited to the Paris area without a clear motivation. Several findings are only explained or mentioned in the text and not shown in tables and figures although this would help the reader to comprehend the discussed findings and conclusions in the paper. CTMs, comparable to the CHIMERE model, are frequently used for simulations covering at least one year, e.g. for emission scenarios studies, this is also reflected in the complexity in such offline coupled CTMS. Therefore it is difficult to compare the value of such 25x25km<sup>2</sup> resolution simulations to the presented 2x2km<sup>2</sup> simulation covering only nine days.

Authors: We address this general comment in the specific points below. The presentation of results has been revised with more validation metrics provided in Table 1. We note however that most of the findings mentioned in the Results Section regard the whole Europe (validation table, budgets, fluxes, exceedances). Only the time series in former Fig. 2 and a quantification of export fluxes was provided for Paris for illustration purposes. While current model validation initiatives indeed rely on annual simulations, there is still scope to document model experiments on isolated case studies, as being done here. A note has been added in the introduction for that respect.

## SPECIFIC COMMENTS

### Introduction

Reviewer #2: Please, add more references in the introduction, e.g. literature on high

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resolution modeling studies over urban areas

Authors: We make a reference to the comprehensive review of Zhang et al., 2012, the reader is referred to this paper and references therein for a wealth of information on high resolution urban scale modelling.

Reviewer #2:l. 15, p. 4191: You mention findings from a 'previous study' which study is that? Please, give a reference.

Authors: We refer to the paper cited later in the sentence : Fountoukis et al. 2013. The sentence has been changed for clarity.

Reviewer #2:Please, add a few more discussion/motivation sentences on the advantages of high resolution modeling on a continental scale.

Authors: A sentence summarizing the main objectives has been added to the first paragraph of the Introduction.

Reviewer #2:Somewhere in the introduction it should be mentioned that the CTM CHIMERE was used in the study.

Authors: The mention has been added.

Reviewer #2:It would be helpful to have a short outline of the paper and a short description of the study (e.g., comparison of 2km simulation to 7km and 50km simulations with CHIMERE focusing on NO2 and PM10 etc.) in the introduction.

Authors: The outline has been added at the end of the introduction.

Reviewer #2:Paragraph 2 (pp. 4190, l. 23 – p. 4191, l. 2): This paragraph is rather isolated; please relate it more to the issue of the paper (high resolution air quality modeling).

Authors: This statement was included to support the need to investigate air pollution in general. However it may be un-necessary in this context and we decided to remove it.

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Reviewer #2: In this paragraph NO<sub>2</sub> and PM<sub>2.5</sub> are explicitly discussed – Are the reasons for showing the modeling results for NO<sub>2</sub> and PM<sub>2.5</sub>/PM<sub>10</sub> mentioned here? If so please mention it somewhere.

Authors: The reason to focus on NO<sub>2</sub> and PM<sub>2.5</sub>/PM<sub>10</sub> is rather that they reached alert levels during that episode as mentioned in the following paragraph.

Reviewer #2: Paragraph 3 (p. 4191, ll. 3 – 18): This time period is explicitly chosen for the simulation mainly due to the high concentrations of several air pollutants resulting from unfavorable weather conditions and resulting high anthropogenic emissions. As the meteorological input fields are not on a higher resolution grid for the 2km simulation the improvement, especially concerning the simulation of peak concentrations, might be underestimated in this simulation. Is the dependence of emissions from meteorological conditions explicitly taken into account in the model? Paragraph 4 (p. 4191, l. 19 – p. 4192, l. 2): The emission inventories are explicitly mentioned as the limiting factor of the horizontal resolution of model, what about the meteorology at that scale?

Authors: We now refer in Section 2.3 to a previous study (Valari and Menut, 2008) to support the approximation of a lower importance of high-resolution for the meteorological driver than for emission. Emissions in relation with residential heating are indeed a function of temperature, a note has been added in the section devoted to emissions (2.4). But resolution is expected to play a limited role on small scale spatial variability of temperature.

## 2 Method

Reviewer #2: Please, add a description of the evaluation method of the simulation presented in this paper, e.g. which stations are used, which pollutants are discussed and why etc. This would help to follow the description of the results in section 3.

Authors: A subsection on observations has been added.

Reviewer #2: One would expect a more detailed description of the CTM as it is in the fo-

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cus of this study. Please, add information on the model set-up (aerosols and chemistry module, vertical resolution, boundary data), e.g. in a table.

Authors: The section on methods has been revised and now includes more details.

Reviewer #2:p. 4192, ll. 12 – 15: Please, describe how the modification for the urban areas is done in the model. How does it influence the model results? Is this method also used in the other model simulations the 2km run is compared to?

Authors: This modified parameterization is described in Terrenoire et al. (2013). A sensitivity analysis will also be presented in the revised version of that paper.

Reviewer #2:p. 4192, ll. 18 – 20: Please, give a short description on how the meteorological fields are interpolated to the CTM grid. Furthermore, one would expect a discussion on the possible impact of the comparable coarse resolution of the meteorological input fields on the CTM simulation results.

Authors: A linear interpolation is used and is now mentioned in Section 2.3. The possible impact of low resolution meteorology is also discussed.

Reviewer #2:Paragraph 3 (p. 4192, l. 21 – p. 4193, l. 27): It is mentioned in this paragraph that a focus of the model development was on improving the emission database. A more detailed discussion on the impact of the high resolution emission database on the model results would be nice to see somewhere in the paper. It could for example include a figure comparing the emitted mass of e.g. primary PM components for the 50 – 7 -2 km simulations.

Authors: More details on the method have been included as well as a map of emissions.

### 3 Results

Reviewer #2:The high spatial grid resolution is expected to increase the model performance among different stations, but in the paper all available stations in the domain or for Paris were averaged. It would be interesting to see whether the model performance

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at individual stations also increases for the high resolution run. (Are there differences in the model performance for different regions?)

Authors: A thorough evaluation of model performance for a 8km resolution over a 1-yr simulation is presented in Terrenoire et al. (2013). We did not consider relevant to duplicate such an extensive analysis for the present experiment that is limited to 10 days, hence less representative.

Reviewer #2: The presented results in this section refer to either the urban or the rural region. So far high resolution simulations are mostly limited to the urban scale. It would be interesting to get more information on what the benefit of the high resolution simulation for the rural regions is and if the interaction between the urban and rural scale is better represented.

Authors: The limited improvement of model performances in rural regions is discussed with the material in Table 1 and the impact of urban pollution export to rural areas is addressed through the example of pollution export out of the city of Paris.

Reviewer #2: One could expect the limited improvement from 7x7km<sup>2</sup> to 2x2km<sup>2</sup> due to the coarse resolution of the meteorology so a more elaborated discussion on this would be desirable.

Authors: See the answer to the comment above on the relative impact of meteorology and emissions.

Reviewer #2: p. 4194 l.12: Why is in Figure 1 the simulation with 2km resolution compared to a simulation with 50km resolution although in the introduction it is mentioned that it is current practice to use a resolution of about 10km (p. 4191, l. 24)?

Authors: This comparison is given for illustration purposes, because this coarse domain is used here as boundary condition of the present experiment and also because such coarse grids were still common practice at European scale only a few years ago (and are still being used for scenario analyses).

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Reviewer #2:p. 4191 l.13: As the high-resolution domain is nested in a coarser (50km) simulation the two domains are not the same. What is the domain of the 2km simulation? This information should be added in the method part.

Authors: This information has been added, the actual domains are plotted in Figure 1.

Reviewer #2:p. 4194, l. 20: Are the individual roads and isolated point sources visible in the fine resolution run? Paragraph 2 (p. 4194, l. 23 – p. 4195, l. 4): Please, give an example for this in Figure 1. Where is this especially relevant?

Authors: The challenge in pointing out such features is to find a location where the source stands out of the background. It is the case for the large point source consisting of oil rigs in the North Sea. Road networks connecting mid-size cities in a clean rural background can be seen in Southern France and Ukraine, although it is difficult to say whether we see the actual road or the conurbation.

Reviewer #2:p. 4195, l. 6: Why is Paris chosen here? Please, add some explanation in the method section. A comparison between different urban areas, cities would be interesting and would support and generalize the findings more.

Authors: The composite time series for Paris is given for illustration purpose but the generalization is discussed in Table 1, which is based on the whole European network of rural/suburban/urban stations.

Reviewer #2:p. 4195, ll. 6 -14: An improvement in the model performance for urban stations has also been described in other studies dealing with high resolution modelling over urban areas. What is the impact at rural stations? A similar figure as figure 2 for rural stations would be nice to see.

Authors: We have added the corresponding time series for suburban and rural stations in former Fig. 2 (now Fig 3).

Reviewer #2:p. 4195, l 15: - How many stations (per urban, suburban and rural station type) are included in the average in table1? Are the results from the model grid the

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station is located in used, or are the model results interpolated to the station location?

Authors: These information were added to Section 2.5 and Table 1.

Reviewer #2: The correlation coefficient in table 1 are hardly mentioned and interpreted in the text. How is the daily spatial correlation coefficients calculated – how can it be interpreted? Could one expect rather an increase of the correlation coefficient for NO<sub>2</sub> at the urban rather than at the rural stations as it is very local? It would also be interesting to include the temporal correlation coefficient based on hourly data.

Authors: More details on the scores and how they are computed are now provided in Section 3.2. A statement has also been added on the improvement of spatial correlation over the whole network, while correlation by station type are somewhat unfavourable by ignoring the improvement in the spatial gradient across monitoring sites. The scores reported in Table 1 refer to daily statistics for which it is not really relevant to compute a temporal correlation given the short time period. Correlation of hourly values are primarily driven by the diurnal cycle of planetary boundary layer mixing and therefore not informative in the present context.

Reviewer #2: p. 4196, l. 4-5: This is an important point which should be taken up and discussed more in the conclusion section.

Authors: A sentence has been added to the conclusion (second paragraph) on the possible perspective for improvement.

Reviewer #2: p. 4196, l. 1: What do you mean with 'netRMSE'?

Authors: That was a typo.

Reviewer #2: p. 4196, ll. 11 – 16: Interesting and important point. It would be clearer if these numbers are presented in a table.

Authors: These numbers have been added to the table.

Reviewer #2: Paragraph 8 (p. 4196 l. 22 – p. 4197 l. 8): Before only results for PM<sub>10</sub>

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and NO<sub>2</sub> are shown, here PM<sub>2.5</sub> is introduced – why?

Authors: PM<sub>2.5</sub> was excluded from the model validation because of the lower number of monitoring sites (due to too stringent data completeness criteria, see answers to Rev #1), but we included it in addition to PM<sub>10</sub> and NO<sub>2</sub> in this revised version.

#### 4 Conclusions

Reviewer #2:p. 4198 I. 1 – 5: These shortcomings should be discussed before.

Authors: This discussion is further detailed in the revised version.

Reviewer #2:p. 4198, II. 7 – 9: Where has this been shown in the paper?

Authors: By investigating export fluxes and budgets, we demonstrated that reduced diffusion was yielding a stronger impact of long range transport and accumulation, which henceforth contribute to the total burden.

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Interactive comment on Geosci. Model Dev. Discuss., 6, 4189, 2013.

**GMDD**

6, C1874–C1882, 2013

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