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Geoscientific Model Development Discussions

Interactive comment on "Frontiers in air quality modelling" *by* A. Colette et al.

Anonymous Referee #1

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GENERAL COMMENTS

The modeling study presented here is an impressive computational achievement and addresses an important question, namely, whether predictions of air quality can be continually improved by using more finely resolved models. However, the paper would benefit from a clearer focus and a more detailed description and explanation of the model setup and results.

Overall, a more detailed technical description of the models used should be provided. (See specific comments below.)

The model evaluation presented in this paper, especially if model evaluation is intended to be a main focus, should be more thorough. I would recommend that the authors expand on their analysis of model performance to include species other than NO2 and PM10 and provide additional discussion regarding the difference in model performance

between urban, suburban, and rural sites.

The scientific results in the paper (i.e., in the direction of "providing new insights for designing air pollution control strategies") also need to be fleshed out further if they are going to be included. If this is the main focus of the paper, then a journal other than GMD (ACP, for instance) may be more appropriate.

SPECIFIC COMMENTS

Anthropogenic emissions inventory.

As the authors mention in the introduction, the spatial resolution of the anthropogenic emission inventory is an important limitation on the horizontal resolution of the model. For this reason, it would be helpful to include more discussion on the emission inventory used here. In the conclusion section the authors refer to "shortcomings in the emission downscaling process." Please identify what these shortcomings are and how they are expected to influence model results. What are the main factors that limit the accuracy of the high-resolution emissions inventory?

I would be interested in seeing a visual comparison between the anthropogenic emissions inventory used in this study and some of the "standard" inventories like EMEP or the high-resolution MACC inventory. For instance, the authors could present a comparison over Paris of what the high, medium, and low inventories look like for selected pollutants (e.g., a plot of emission rate superimposed over a map).

Model domain and setup.

In the Methods section, the authors explain that the CHIMERE model with 2km resolution is driven by meteorological fields with only 16km resolution. This mismatch requires significantly more discussion. How do the authors expect this difference to manifest itself in the model results? What, if any, spurious effects do the authors expect to see based on this mismatch? Can the authors provide any insight into these questions based on their own experiences with CHIMERE, or based on any published

model studies?

I am not clear on the geographical bounds of the model domain based on the current text. The authors should specify the latitude and longitude bounds on the high-resolution domain and the coarser (50 km) domain, as discussed at lines 11-12 of page 4194. A simple figure showing the domains on a map might also be helpful.

Please specify what model timestep was used in the run and clarify the statement beginning at p. 4194, line 28: "The computational demand of the simulation presented here is thus two orders of magnitude (5×5 for the number of horizontal points, and another factor 5 for the increment in the time step) above current practices." What is the model timestep in the present study and what do the authors consider "current practice?"

At around line 10 of page 4195 and in Table 1, the authors introduce a comparison between coarse, 7km resolution and 2km resolution model runs. It would be helpful if these different model runs were introduced earlier. What are the domains for each? Are each forced by the same 16-km ECMWF-IFS model data? What emissions inventory data sets were used for each run?

In the description of the computing power needed for this model simulation (e.g., at the top of page 4194), please also indicate how much real time and how much memory it took to run the job.

Model evaluation.

The authors do not explain why NO2 and PM10 are the only chemical species to be evaluated in this paper. Since PM2.5 exceedances and fluxes are discussed later in the results section, PM2.5 should also be included in the discussion of model evaluation (e.g., in Table 1). I recommend that the authors also evaluate modeled O3 concentrations; evaluations of additional chemical species could also add depth to the analysis.

Comparison of modeled vs. measured time series for the modeled air pollution episode

is only shown for NO2 in Paris. I would be interested in seeing similar figures for other sites – a comparison of urban, suburban, and rural sites, for instance. Time series for species other than NO2 would also be interesting.

Table 1 should include the mean of the observed and modeled values in concentration units. Please also present a metric that includes the direction of the error (i.e., whether modeled values are too high or too low) in addition to the RMSE, which only gives information about the magnitude of the error. For instance, mean bias or mean fractional bias would be appropriate. For the statistical metrics used in Table 1, please include a definition of how each metric was calculated (in a footnote or an appendix would be fine.)

Scientific results.

As the authors highlight in the conclusion, some of the most interesting scientific results of the study are those related to exceedances of air quality standards and net fluxes of pollution. However, these aspects are given very little space in the results section; if they are going to be included there needs to be an expanded description and discussion of both.

For exceedances – it would be nice to see results in a table as you have done for concentration predictions in Table 1. The authors mention PM2.5 exceedances (although results for PM2.5 are not shown anywhere else in the paper). What about exceedances for other species?

For net fluxes – the authors need to explain how the "net outgoing flux of traces species" is calculated. What are the model parameters used in this calculation? Is the calculation done gridcell by gridcell around the perimeter of the Paris region and then summed? What model vertical layers are being considered? Related to these fluxes, the authors write in their abstract that "the high resolution grid also allows revisiting the contribution of individual city plumes to the European burden of pollution." To support this statement, the authors should present quantitative values for this "contribution of

individual city plumes to the European burden" in their paper. For instance, in their simulation, what was the net export of PM2.5 from the Paris region in absolute terms? Currently only relative values comparing the model runs are presented (e.g., the net export of PM2.5 was greater in the high-resolution simulation than in the coarse simulation).

The abstract also indicates that the model provides "new insights for designing air pollution control strategies." This seems somewhat overstated; the authors should specify what they see as the insights for air pollution control strategies and how they are derived from the model results. It seems that what is currently argued is that such a high resolution model could be a potential tool for policy makers.

TECHNICAL CORRECTIONS

Line 3, p. 4191. Should be "One of the main air pollution outbreaks. . ." rather than "outbreak"

Line 19, p. 4191. I think "proposed" should be replaced by "used."

Line 22, p.4195. Should be "latter" rather than "later."

Line 26, p. 4198. Should be "citizens" rather than "citizen."

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