Geosci. Model Dev. Discuss., 6, C1867–C1873, 2013
www.geosci-model-dev-discuss.net/6/C1867/2013/

Geoscientific Model Development Discussions
Open Access

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

A. Colette et al.

augustin.colette@ineris.fr

Received and published: 2 November 2013

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 #1: A more detailed technical description of the models used should be provided.

Authors: More details on the Chimere setup and on the emission preprocessing are now available in the paper. The section on methods has been divided between the introduction of the model, physical and chemical model setup, input data (meteorology and emissions), computational setup, and observations used in the validation.

Reviewer #1: The model evaluation presented in this paper, especially if model eval-

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

uation 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.

Authors: Following the guidance of the editor, the paper has been moved from the model evaluation to the model experiment category. Nevertheless we agree that the evaluation could be more detailed. Therefore, a validation for PM2.5 has been added, but Ozone was not considered relevant for this January case study. The number of stations used in the validation was also increased, for the original submission we used a subset of data designed for trend analysis that had stringent criteria on data completeness that were not relevant here. Therefore all the numbers in Table 1 were revised. A discussion of the performances according to the station typology was already included in the submitted text and Table 1 but further details have been added.

SPECIFIC COMMENTS

Anthropogenic emissions inventory.

Reviewer #1: 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.

Authors: The section on emission data has been re-written and further details were added to improve this.

Reviewer #1: 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?

Authors: As detailed in the section on emissions, a significant work was performed

GMDD

6, C1867–C1873, 2013

to improve the top-down spatialisation of emission that are originally reported at 0.5 degree resolution in EMEP data. This downscaling remains however based on EU-wide proxies (e.g. the same correlation coefficient between population density and emission gradients is applied throughout Europe) and reporting national emission at higher resolution (e.g. 10km) would greatly help.

Reviewer #1: 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).

Authors: A map has been added for PPM2.5. It should be stressed, as explained in the section on emission, that the present inventory is a hybrid of EMEP national totals and MACC spatial patterns, plus some refinement for industrial and residential emissions.

Model domain and setup.

Reviewer #1: 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?

Authors: It is indeed unfortunate that we could not include a 2km resolution meteorological simulation for the present project, because the (already high) computational cost would have more than doubled. On the expected impact of this approximation, we now refer in the text to Valari and Menut, 2008 who compared the added value of resolution for both meteorology and emissions and find a clear domination of the latter.

GMDD
6, C1867–C1873, 2013

Reviewer #1: 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.

Authors: The actual full model domain are those represented in Figure 2. A note has been added in the model setup section.

Reviewer #1: 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?"

Authors: We refer to the common practice in the context of the introduction (where this sentence is extracted from) where we discuss MACC forecasts. The factor 5 is the approximate ratio between MACC (10km) and this work (2km), although of course the challenge is different when it comes to producing daily forecasts than analyzing a single pollution episode. This factor 5 difference yields two order of magnitudes (actually a factor 125) in computing demand considering the increment in space (number of points) and the corresponding increment in time (CFL criteria). The time step used has been added to the model description.

Reviewer #1: 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?

Authors: The section devoted to the methods has been substantially revised to better introduce all the model runs, see specifically the revised "model setup" part (2.2). All

GMDD
6, C1867–C1873, 2013

three simulations are driven by the same IFS forcing, interpolated linearly at the appropriate resolution. The same emission fluxes are used for all three simulations, but they are spatialised at the appropriate resolution according to the downscaling method. Sections 2.3 and 2.4 are revised accordingly.

Reviewer #1: 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.

Authors: The wall clock time (5h40 for 24h simulated) has been added to the text in Section 2.6. Unfortunately we did not keep any information on the memory used.

Model evaluation.

Reviewer #1: 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.)

Authors: The validation of the model for PM2.5 has been added to Table 1 as well as

GMDD
6, C1867–C1873, 2013

the mean fractional bias. However we decided not to include ozone that is not really relevant for this wintertime period. Particulate matter (both PM2.5 and PM10) is only available on a daily basis for most sites in Europe. Therefore we decided not to include time series or temporal correlations given the short time period investigated here. For an annual validation of the 8km model setup that includes temporal statistics the reader is referred to Terrenoire et al. (2013).

Scientific results.

Reviewer #1: 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?

Authors: As suggested, the statistics on exceedances have been added to Table 1. In addition to exceedances of the daily PM10 limit value (50μg/m3) already discussed in the paper, the hourly NO2 limit value (200μg/m3) has been added. Remaining limit values (e.g. for PM2.5) defined in the EU 2008 Directive regard annual statistics and were therefore not relevant here.

Reviewer #1: 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 "contri-

GMDD
6, C1867–C1873, 2013

bution 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).

Authors: A revised explanation of the method used to compute fluxes is given. The reference was also changed to use the 2km resolution setup as a basis for the relative differences of fluxes at 57 and 8km (while in the previous version, the coarse domain was used as a reference).

Reviewer #1: 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.

Authors: We conclude by saying that further development are needed before we can confidently rely on such high resolution simulations. Rather, the point on the policy relevance in the abstract consist in highlighting possible limitations in the model setups now being used in the Decision making that rely on 0.5degree emission data officially reported by the countries. A specific reference to the underestimation of export fluxes is made since it sheds some light on the level of action of mitigation (at the local or national scale).

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